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Death and Dying in Puritan New England

DAVID E. STANNARD

It is now almost a cliché among historians to acknowledge that “tensions” arising from the seemingly paradoxical structure of much of the Puritan faith were an unavoidable and essential element of that faith. Expanding on a theme outlined by Max Weber over half a century ago, recent historians have found these tensions rooted in such matters as the Puritans’ contempt for the merits of social virtue—“merit-mongering,” Cotton Mather called it—while they simultaneously insisted on proper social conduct, and in their rejection of the ultimate importance of earthly material accomplishments at the same time that they sanctified work.¹ A substantial list of such contrasts might easily be compiled, the tensions apparent, to greater and lesser degrees, in many areas of their life and thought.² Principally, however, Weber observed, and historians have concentrated on, tensions between religious belief and worldly activity—between religion and economics, religion and education, religion and art, religion and the community. Less often noted are the tensions that existed within the religion itself between religious ideal and religious experience, tensions that resided solely within the religious sphere of the Puritans’ lives but which may well have affected the very structure and durability of their culture.

The vision of death and the act of dying were to the Puritans profoundly religious matters. Much of the average Puritan’s life was centered about and predicated on the vision of death, the afterlife, and the expected manner in which the passage from this world to the next should be made. So it was, and is, with most intensely religious communities. But in the Puritan scheme something was wrong, as though an improper “fit” was made between

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¹ For the most direct example of this aspect of Weber’s thought, see his “Religious Rejections of the World and Their Directions,” in H. H. Gerth and C. Wright Mills, eds., *From Max Weber: Essays in Sociology* (New York, 1946); Cotton Mather, *Parentator* (Boston, 1724), 185.

² A collection of readings has recently been organized about this theme. See Richard Reinitz, *Tensions in American Puritanism* (New York, 1970).

the vision of death and the manner of encountering it. The resulting tension, I will argue, haunted the devout Puritan throughout his life and grew particularly intense as death approached—and it is only the devout Puritan, not the entire populace of seventeenth-century New England, that is of concern here. But before considering this tension further and speculating on its larger cultural ramifications, it is worth considering the context out of which the Puritan experience emerged.

MEN HAVE ALWAYS feared death, as Bacon wrote, “as children fear to go in the dark.” The handling of this fear, the attempt somehow to mitigate it, is one of the ways cultures have of distinguishing themselves from one another. Thus among tribes of hunters who, as Joseph Campbell puts it, “live in a world of animals that kill and are killed and hardly know the organic experience of a natural death,” death is regarded as externally caused and is strenuously resisted by magic; while the Greeks saw death optimistically, indeed, as the beginning of life, “at least,” as Herbert Marcuse has observed, “for the philosopher.” In ancient Taoist China death was given its place in the “general attitude toward the universal laws of nature, which is one not merely of resignation nor even of acquiescence, but a lyrical, almost ecstatic acceptance”; while the Australian Aranda attempt to keep the spirit at bay by burning the village whenever a death occurs, by committing violent assaults on the grave itself, and by refusing ever again to utter the name of the deceased. Among the Javanese death is treated with a stoic calm, the body of the deceased being disposed of as quickly as possible, often within an hour or two; while in much of modern America cosmetically “restored” corpses lie in so-called slumber rooms for days—a few are even frozen—as acquaintances of the deceased make ritualized public showings of grief.³

In each of these cases the behavior in the face of dying is the result of the attitude toward, and the vision of, death. To use the language of the anthropologist, the *ethos*, “the tone, character, and quality of . . . life, its moral and aesthetic style and mood,” grows out of the *world view*, that is, the “picture of the way things, in sheer actuality are, [the] concept of nature, of self, of society.” The influence, of course, is mutual. Suggesting a certain synonymity between these terms and religious “belief” (world view) and “ritual” (ethos), Clifford Geertz writes: “Religious belief and ritual confront and mutually confirm one another; the ethos is made intellectually reasonable by being shown to represent a way of life implied by

³ Francis Bacon, *The Essays, or Councils, Civil and Moral* (London, 1718), 3; Joseph Campbell, *The Masks of God: Primitive Mythology* (New York, 1959), 125–26; Herbert Marcuse, “The Ideology of Death,” in Herman Feifel, ed., *The Meaning of Death* (New York, 1959), 67; Arthur Waley, *The Way and its Power* (New York, 1949), 54–55; Baldwin Spencer and F. J. Gillen, *The Native Tribes of Central Australia* (London, 1899), 497–511; Clifford Geertz, *The Religion of Java* (New York, 1960), 68–76.

the actual state of affairs which the world-view describes, and the world-view is made emotionally acceptable by being presented as an image of an actual state of affairs of which such a way of life is an authentic expression."⁴ The two phenomena, ethos (in the present case, the prescribed way of dying) and world view (the vision or concept of death) reinforce one another and thus fused give meaning, order, and stability to their cultural source.

As all cultures must, the early Christians formulated methods of dealing with the fact of death. Prior to the Christian era the Homeric Greeks had devised the concept of the soul, an idea that grew out of an earlier belief that the dead continued to live under the earth—the later vision of the disembodied spirit thus opening the way to the practice of cremation. It was not until the worship of Dionysius, however, and in the writing of Heraclitus, that the belief in the soul's immortality reached its full development. This answer to the fear of the void of death became a central tenet of Christian doctrine in the idea of the Resurrection. "I am the resurrection and the life," Christ had said, "he that believeth in Me, although he be dead shall live: and everyone that liveth and believeth in Me shall not die forever." Thus in the fourth century Augustine could say of the death of his mother, "We thought it not fitting to solemnise that funeral with tearful lament, and groanings; for thereby do they for the most part express grief for the departed, as though unhappy, or altogether dead; whereas she was neither unhappy in her death, nor altogether dead." And Saint Ambrose, in his oration at the funeral of Valentinian, says:

But if the gentiles, who have no hope of resurrection, are consoled by this alone, in that they say that after death the departed have no life and consequently no sense of pain remains, how much the more should we receive consolation because death is not to be feared, since it is the end of sin, and that life is not to be despaired of which is restored by the resurrection?⁵

In place of fear of the void, however, there evolved the fear of divine wrath, of punishment for sin; for along with the concept of immortality, Christianity devised a place of bliss and a place of misery as the potential residences of the soul. For the saved there was heaven, for the unrepentant hell, and eventually, for the great masses who had not yet been thoroughly cleansed of minor sins, there was purgatory, where, as Hamlet's ghostly father described it, the soul was

confined to fast in fires,
Till the foul crimes done in my days of nature
Are burnt and purged away.

⁴ Clifford Geertz, "Ethos, World-View and the Analysis of Sacred Symbols," *Antioch Review*, 17 (1957-58): 421, 422.

⁵ E. Rohde, *Psyche, The Cult of Souls and Belief in Immortality Among the Greeks* (New York, 1925), 19-24, 253-56; Bruno Snell, *The Discovery of the Mind*, tr. T. G. Rosenmeyer (New York, 1960), 17-18; John 11:25-26; Augustine, *Confessions*, tr. Edward B. Pusey (New York, 1949), 190; Ambrose, *De Consolatione Valentiniani*, 45, quoted in Alfred C. Rush, *Death and Burial in Christian Antiquity* (Washington, 1941), 11-12.

While hell was clearly a place to be avoided, and although the temptations of Satan were many and great, the sinful were afforded a variety of ways through which they might avoid that fate. Baptism cleansed the soul of original sin, confession and the administration of the Eucharist throughout life prepared the soul for heaven, the sacrament of extreme unction and the viaticum at death further cleared the way, and even while the imperfect but uncondemned soul lingered in purgatory, indulgences, requiem masses, and the prayers of the living helped improve the likelihood of imminent removal to heaven. Thus the fate of the individual was largely in his own hands and those of his family and friends; hell was a fearsome end for the soul, but an end that could, with a little work, be avoided. It is not surprising, then, to find the early Christian concept of death reflecting the optimistic *Migratio ad Dominum*, an image perhaps given its fullest previous expression among the ancient Egyptians.⁶

In effect the Christian world view (belief) and ethos (ritual) had successfully fused: supramundane existence was composed of God, of heaven, hell, and purgatory, and of the souls of men assigned their place principally on the basis of their earthly behavior; at the same time the institutions, from baptism to the requiem mass, had been created to make such a world view emotionally and intellectually acceptable. In theory at least, a believing and practicing Christian was armed against fear when death approached. A culturally functional concept of death had been constructed and made viable; now it had only to be maintained.

Fear, of course, still plagued men; even Christians. In Europe during the late Middle Ages it appears to have reached a peak of intensity, probably due in large measure to the devastating plagues that wracked that part of the world during the fourteenth and fifteenth centuries, though other possible explanations have been offered.⁷ It was during this period that the *danse macabre* and *Ars Moriendi* traditions flourished, that tomb sculpture turned toward literal representations of the deceased in advanced stages of decomposition, that indeed, as Theodore Spencer has put it, "a great poet, like Villon, had nothing but death to write about." "It is hardly an exaggeration," Spencer writes, "to say that in Northern Europe the whole fifteenth century was frenzied about death."⁸

⁶ See Rush, *Death and Burial*, 44-71.

⁷ Theodore Spencer, for example, attributes much of the concentration and consternation concerning death in this period to the development of artistic realism and to an attendant rise in emotional identification with the death of Christ, that favorite artistic motif of that and virtually every other era. See Spencer, *Death and Elizabethan Tragedy* (Cambridge, Mass., 1936), 19-21.

⁸ On the *danse macabre*, see Leonard P. Kurtz, *The Dance of Death and the Macabre Spirit in European Literature* (New York, 1934); on the *Ars Moriendi* see Sr. Mary C. O'Connor, *The Art of Dying Well: The Development of the Ars Moriendi* (New York, 1942); Erwin Panofsky, *Tomb Sculpture: Its Changing Aspects from Ancient Egypt to Bernini* (London, 1964), 63-66; cf. T. S. R. Boase, *Death in the Middle Ages* (London, 1972), 96-103; Spencer, *Death and Elizabethan Tragedy*, 32.

Frenzied, perhaps; but a controlled frenzy, a frenzy that operated within the confines of the Christian scheme. The primary medieval concern was with the physical horrors of death. The *danse macabre*, as Johan Huizinga has noted, was "a dance of the dead and not of Death." Its principal purpose was to "remind the spectators of the frailty and the vanity of earthly things . . . while at the same time [it] preached social equality as the Middle Ages understood it, Death levelling the various ranks and professions."⁹ Similarly the poetry of Villon and others, while it concentrated on death, was most typically rendered in passages like the following:

Death makes him shudder and turn pale,
The nose to curve, the veins to swell
The neck to inflate, the flesh to soften
Joints and tendons to grow and swell.¹⁰

And the *Ars Moriendi*, "in spite of its purpose," Mary C. O'Connor notes, "is not a doleful book—no clarion call to repentance. There is little stress upon hell, only hope of heaven. Always is *Moriens* encouraged and consoled."¹¹ Death, in the late Middle Ages, was a ghastly visitation upon the *body* of man, but fear of the soul's fate remained blunted by the Christian tradition. This is clearly seen in the popular belief of the period that, following his resurrection, Lazarus lived in constant torment that he might ever again have to endure the physical act of dying. As Huizinga observed: "The dominant thought, as expressed in the literature, both ecclesiastical and lay, of that period, hardly knew anything with regard to death but these two extremes: lamentation about the briefness of all earthly glory, and jubilation over the salvation of the soul."¹²

Christian optimism, the principal weapon against the fear of death, was not shaken by the morbidity of the Middle Ages, by, as T. S. R. Boase has recently put it, this "strange preoccupation with putrefaction."¹³ Indeed by the middle of the sixteenth century it is not at all surprising to find John Haryngton, for example, writing:

Death is a porte whereby we pass to joye;
Lyfe is a lake that drowneth all in payne;
Death is so dear, it killeth all annoye;
Lyfe is so lewd, that all it yields is vayne.¹⁴

And by the time the seventeenth century was under way, among the more typical responses to death were those of William Drummond, who

⁹ Johan Huizinga, *The Waning of the Middle Ages* (London, 1952), 131.

¹⁰ Villon, quoted in Huizinga, *Waning of the Middle Ages*, 133.

¹¹ O'Connor, *Art of Dying Well*, 5.

¹² Huizinga, *Waning of the Middle Ages*, 132.

¹³ Boase, *Death in the Middle Ages*, 106.

¹⁴ Quoted in Spencer, *Death and Elizabethan Tragedy*, 53.

saw it as “but a short, nay, sweet sigh; and . . . not worthy the remembrance”; of Jeremy Taylor, who wrote that “it is so harmless a thing, that no good man was ever thought the more miserable for dying, but much the happier”; or Sir Thomas Browne who, because of the expected negative reactions of others—even “the Birds and Beasts of the field”—to his physical remains, was “not so much afraid of death, as ashamed thereof.”¹⁵

The seventeenth century, however, did not belong entirely to the Drummonds, the Taylors, and the Brownes; it was also the century of the Mathers, the Hookers, and the Willards—the century of New England Puritanism. The Puritans, though heirs to the Christian tradition, sought to purify that tradition as it had evolved in their time, to cut through the extraneous trappings of the formal Church and revive the spirit of the earliest Christians. With this in mind they had closely examined the teachings of the Church of which they were a part, the Anglican, and had challenged many of those teachings, and in the end some of them sought a richer environment for their work in the rocky soil of New England.

“SAVING GRACE,” as the Puritans called it, was crucial to their quest for salvation and was imparted by a power beyond coercion. Without it, regardless of how good one may have been in mortal life, there was simply no possibility of moving to one of the most critical levels in the Puritan morphology of conversion, the level of assurance. “The Lord to shew the sovereign freedom of his pleasure,” Thomas Hooker wrote, “that he may do with his own what he wil, and yet do wrong to none, he denyes pardon and acceptance to those who seek it with some importunity and earnestness . . . and yet bestowes mercy and makes known himself unto some *who never sought him*.”¹⁶ Assurance, this important step up the steep ladder of salvation, was thus a step fraught with much difficulty and confusion. “It is a great exercise to some Saints,” noted Solomon Stoddard,

whether they be sincere Saints, they labour in it for many years; and one Minister gives signs, and they try themselves by them, and another gives signs, and they try themselves by them; and sometimes they think they see the signs of Saints, and sometimes the signs of hypocrites: and they dont know what to make of themselves.¹⁷

Once assurance was attained, however, once God had provided some evident sign of saving grace, the believer was accepted by the community and himself as a Visible Saint, that is, as having been saved. Assurance nonetheless did not guarantee salvation. Men could both deceive and be deceived,

¹⁵ William Drummond, *A Cypress Grove* [1623] (London, 1909), 69; Jeremy Taylor, *The Rule and Exercises of Holy Dying* [1651] (New York, 1869), 95; Sir Thomas Browne, *Religio Medici* [1643] in Browne, *Works*, ed. Charles Sayle (Edinburgh, 1912), 1: 58.

¹⁶ Thomas Hooker, *The Application of Redemption* (London, 1657), 299.

¹⁷ Stoddard, *The Tryal of Assurance* (Boston, 1699), 17.

and thus there was always a lingering doubt—indeed, a necessary doubt. “Even after he reached the stage of assurance,” Edmund S. Morgan has observed, “his doubts would continue. If they ceased, that would be a sign that he had never had faith to begin with, but had merely deluded himself and had not really entered into the covenant of grace.”¹⁸ The cessation of doubt was a sign of “security,” of that false assurance of which, in 1629, Arthur Hildersam wrote,

for one that Sathan hath overthrowne by desperation, there are twenty whom he hath overthrowne with this false assurance. Wee are therefore to be exhorted to examine our assurance. . . . For, as the true assurance of Gods favour, is a comfortable thing; so is a false peace and assurance one of the most grievous judgements that can befall a man. . . . Of the two, it were better for a man to be vexed with continuall doubts and feares, than to be lulled asleepe with such an assurance. For, besides that it keeps a man from seeking to God, it will not hold, but faile him, when he shall have most neede of it.¹⁹

The roots of this necessary doubt were deep, and the effects wide-ranging and persistent. C. C. Goen has called it “one of the most vexing problems in the Great Awakening.” As Jonathan Edwards commented, over a century after Hildersam, on the behavior of “the greater part” of newly converted Saints:

They generally have an awful apprehension of the dreadfulnes and undoing nature of a false hope; and there has been observable in most a great caution, lest in giving an account of their experiences, they should say too much, and use too strong terms. And many after they have related their experiences, have been greatly afflicted with fears, lest they have played the hypocrite, and used stronger terms than their case would fairly allow of; and yet could not find how they could correct themselves.²⁰

Thus for as long as he lived even the most apparently obvious candidate for Sainthood did not dare take his election for granted; there was no way in this world of knowing with certainty whether one was saved or not. The best sign of assurance, in other words, was to be unsure. As a result the devout Puritan constantly examined himself and assailed every evidence of impurity, filling journals and diaries with interminable exhortations on the depravity of all men, most of all himself. For as Jonathan Edwards put it, “There is no man on earth, that is so just, as to have attained to such a degree of righteousness, as not to commit any sin.” Wickedness, “if divine grace does not prevent . . . may as truly be said to be the effect which man’s natural corruption tends to, as that an acorn in a proper soil, truly tends by

¹⁸ Edmund S. Morgan, *Visible Saints* (Ithaca, 1963), 69. Cf. Norman Pettit, *The Heart Prepared: Grace and Conversion in Puritan Spiritual Life* (New Haven, 1966), 18–19.

¹⁹ Arthur Hildersam, *Lectures upon the Fourth of John* (London, 1629), 311.

²⁰ Jonathan Edwards, *A Faithful Narrative* [1737], reprinted in *The Works of Jonathan Edwards*, ed. C. C. Goen, 4 (New Haven, 1972): 186. The Goen quote is from *ibid.*, 47; but see also his *Revivalism and Separatism in New England, 1740–1800* (New Haven, 1969), 44–54.

its nature to become a great tree." The Puritan faith, upon one tenet of which John Preston had pronounced in 1633, "this is a very comfortable doctrine, if it be well considered," was instead a faith marked by a never-ending, excruciating uncertainty. "Surely if ever a theology tortured its votaries," Perry Miller has written, "it was that taught by New England divines, and if ever mortal was driven to distraction it was the mother who, as Winthrop tells, drowned her child that it might escape damnation."²¹

An equally critical ambivalence was built into the Puritan view of death. On the one hand, life was seen as but a "vapour," a fleeting "pilgrimage"—the latter word apparently taken from the eleventh chapter of Hebrews, where it is applied to Abraham and his descendants.²² It is common to come upon such references in Puritan writing as Increase Mather's mention of "the dayes of my pilgrimage now drawing to their close," of his wife as "the Dear Companion of my Pilgrimage on Earth," or of John Collins offering a book to the reader as being "peculiarly suited to the support and consolation of the Saints in this their wayfaring and afflictive pilgrimage." It was in the afterlife that the Saints were to be rewarded and the sinful punished. As Collins wrote: "Death is only sweetened to us as we can look upon it our priviledge; as an out-let from sin and misery, and an in-let to *Glory* both in Holiness and Happiness."²³

Collins's attitude was neither uncommon nor new. More than a century earlier William Perkins described death as "a blessing . . . as it were a little wicket or doore whereby we passe out of this world and enter into heaven." Indeed Increase Mather once wrote after contemplating the beauty of the soul's flight to heaven, "the thought of this should make the Believer long for death."²⁴ The desirability of such longing was given Biblical justification, most frequently 2 Corinthians 5:6–8: "Being therefore always of good courage, and knowing that, whilst we are at home in the body, we are absent from the Lord (for we walk by faith, not by sight); we are of good courage, I say, and are willing rather to be absent from the body, and to be at home with the Lord."

All of this was, to be sure, consistent with the long Christian tradition already observed. The Bible does, however, have other things to say about

²¹ Jonathan Edwards, *Original Sin*, in Edwards, *Works* (New York, 1881), 2: 328; John Preston, *The New Creature* (London, 1633), 23; Perry Miller, *The New England Mind: The Seventeenth Century* (Boston, 1961), 56. See also, Michael McGiffert, ed., *God's Plot: The Paradoxes of Puritan Piety* (Amherst, Mass., 1972), esp. 10–29, for an insightful outline of what the author calls the "dialectic of contraries" involved in the phenomenon of Puritan Sainthood.

²² For a full discussion of the term, see the research of Albert Mathews in *Colonial Society of Massachusetts, Publications*, 17 (1915): 300–92.

²³ Increase Mather, *The Mystery of Christ Opened and Applied* (Boston, 1686), 2; Mather, *A Sermon Concerning Obedience & Resignation to the Will of God* (Boston, 1714), 38; John Collins, "To the Reader," in Jonathan Mitchel, *A Discourse of the Glory* (2d ed.; Boston, 1721), 2, 4–5.

²⁴ William Perkins, *Salve for a Sicke Man* (Cambridge, 1597), 5; Increase Mather, "Preface," in Mitchel, *Discourse of the Glory*, iv.

death, things present but less emphasized in earlier Christian thought. Among the more important passages is Romans 5:12: "Therefore, as through one man sin entered into the world, and death through sin; and so death passed unto all men, for that all sinned." The reference to "one man," of course, is to the story of Genesis. It was not lost on the Puritans. Whenever they listed the "stings of death," as Leonard Hoar did in 1680, the very first was that death is punishment for sin. "The first sting of death," Hoar wrote, "is that it came into the world through man's own fault. . . . It is sin brought in death as a curse and punishment. Death comes from God, not as instituting the course of Nature at first; but as revenging sin." Indeed death, Hoar writes, "is the greatest evil in the world." Again and again the image of death as a dreadful punishment for the sin of Adam surfaces in Puritan writing, from the simple tombstone carving: "Death which came on man by the fall,/ cuts down father child and all" to the eloquence of Jonathan Edwards:

For death, with the pains and agonies with which it is usually brought on, is not merely a limiting of existence, but is a most terrible calamity; and to such a creature as man, capable of conceiving of immortality, and made with so earnest a desire after it, and capable of foresight and of reflection on approaching death, and that has such an extreme dread of it, is a calamity above all others terrible, to such as are able to reflect upon it. . . . It is manifest, that mankind were not originally subjected to this calamity. . . . Sin entered into the world, and death by sin, as the apostle says.²⁵

The ambivalence inherent in such a dual concept—that death is in a sense both punishment and reward, and that those facing it should regard it as one or the other—is evident in virtually every Puritan funeral sermon or other discourse on the subject.²⁶ Somehow, although the formal resolution of this conflict through the intercession of the death of Christ was acknowledged by Puritans at least as early as Perkins,²⁷ their constant references to the viability of the earlier view of death as punishment clearly belies any total, exclusive satisfaction with either image.

Whatever the specific image of the moment, death as a phenomenon seems to have been one of the more important preoccupations of the devout Puritan, as it was in the late Middle Ages. Although not exposed to an inordinate amount of death when compared with his relatives in England—in fact, it is probable that he lived a comparably healthier and longer life²⁸—

²⁵ Leonard Hoar, *The Sting of Death* (Boston, 1680), 4, 3; epitaph from the Hull stone, Cheshire, Connecticut, cited in Allan I. Ludwig, *Graven Images* (Middletown, Conn., 1966), 88; Edwards, *Original Sin*, 372.

²⁶ For a particularly clear example of the confusion wrought by this ambivalence, see Urian Oakes's long poem, *An Elegie Upon the Death of the Reverend Mr. Thomas Shepard* (Boston, 1677).

²⁷ Perkins, *Salve for a Sicke Man*, 4-5; see also his *Golden Chaine* (Cambridge, 1597), 168-72.

²⁸ Cf. C. W. Chalkin, *Seventeenth-Century Kent* (London, 1965), 33-41; Peter Laslett, *The World We Have Lost* (New York, 1965), ch. 5; John Demos, *A Little Commonwealth* (New York, 1970), 65-66, 192-93; Philip J. Greven, Jr., *Four Generations* (Ithaca, 1970), 24-29, 106-10, 269; Kenneth A. Lockridge, *A New England Town* (New York, 1970), 67-69.

it was the unquestioned duty of every right-thinking Puritan to keep the thought of death ever on his mind. "A prudent man," Cotton Mather wrote, "will *Dy Daily*; and this is one Thing in our doing too: Tis to *live Daily* under the power of such Impressions, as we shall have upon us, when we come to Dy. . . . Every Time the *Clock Strikes*, it may *Strike* upon our Hearts, to think, *thus I am one Hour nearer to my last!* But, O mark what I say; That *Hour* is probably *Nearer* to None than to such as *Least Think* of it." Later, in *Death Made Easie & Happy*, he urges the reader to remind himself daily "that he is to die shortly. Let us look upon everything as a sort of Death's-Head set before us, with a *Memento mortis* written upon it."²⁹ Even children were early immersed in the required preoccupation of their elders, whether learning the alphabet and encountering such rhymes as: "G—As runs the *Glass* / Man's life doth pass; T—*Time* cuts down all / Both great and small; X—*Xerxes* the great did die, / And so must you & I; Y—*Youth* forward slips / Death soonest nips,"³⁰ or obeying the advice of an esteemed teacher like Joseph Green: "Remember Death; think much of death; think how it will be on a death bed."³¹ One has only to leaf casually through the pages of Samuel Sewall's *Diary* to see repeated examples of this preoccupation in action.

But what of it? What do these tensions and preoccupations add up to? What do they mean? Few writers have had anything to say about the Puritan encounter with death, but this should not be surprising; neither has there been, until very recently, much psychoanalytic work on the subject of death in any sense. One psychiatrist who has investigated the subject at some length suggests that "psychiatrists, no less than other mortal men, have a reluctance to consider or study a problem which is so closely and personally indicative of the contingency of the human estate."³² Whether he is accurate in this assessment, or whether it applies to historians, is of minor importance here; what is important is that, for whatever reason, there is a definite paucity of literature on the subject. Nevertheless, within the small body of writing that does exist there appears the general impression that the Puritans confronted death optimistically, with neither doubts nor fears. Perry Miller, for example, directs himself to what he terms the "cosmic optimism" of the Puritans in facing all manner of adversity, then notes the relatively "few sermons specifically devoted to immortality compared with the tremendous number drawing out the lessons of depravity or analyzing in minute detail the processes of regeneration. Perhaps the expectation of immortality was so

²⁹ Cotton Mather, *The Thoughts of a Dying Man* (Boston, 1697), 38–39; see also his *Awakening Thoughts on the Sleep of Death* (Boston, 1712), 16–20, for similar thoughts and phrasing; Mather, *Death Made Easie & Happy* (London, 1701), 94.

³⁰ *The New England Primer* [1727] in facsimile copy of the reprinted 1897 edition, ed. Paul Leicester Ford (New York, 1962).

³¹ Joseph Green, *The Commonplace Book of Joseph Green* (1696), ed. Samuel Eliot Morison, Colonial Society of Massachusetts, *Publications*, 34 (1943): 204.

³² C. W. Wahl, "The Fear of Death," in Feifel, *Meaning of Death*, 19.

axiomatic," he surmises, "that little discussion was needed, but I am inclined to suspect that because their energies were so intensely concentrated upon the problems in hand they had few left for doubts about those to come." Or, as Allan I. Ludwig has more recently, and somewhat more dramatically, put it: "In the midst of darkness and confusion there was light, the triumph of Death was overcome by eternity. The fear of death gave way to the thrill of spiritual pleasures yet to come as archangels trumpeted the glorious day."³³

The evidence does not confirm this interpretation. Instead it suggests that the Puritans were gripped individually and collectively by an intense and unrelenting fear of death, while simultaneously clinging to the traditional Christian rhetoric of viewing death as a release and relief for the earth-bound soul. Increase Mather provides a clear-cut example of this duality. Mather was fond of the kind of declaration cited earlier, indicating that believers should long for the deliverance of death. Indeed in one of his sermons published in 1715 he cried: "I know that the time of my departure out of this World is now very near at hand. . . . And now that I am *Preaching Christ*, how glad should I be, if I might dye before I stir out of this pulpit!"³⁴ But eight years later, when death was in fact near at hand, his reaction was quite different. As his son Cotton relates and explains it:

And in the Minutes of the Darkness wherein he lay thus *feeble and sore broken*, he sometimes let fall expressions of some *Fear* lest he might after all be Deceived in his *Hope* of the *Future Blessedness*. His Holy Ministry having very much insisted on that Point, that *no care could be too much to prevent our being Deceived in that Important Matter*; tis no wonder, that as the *Dark Vapours* which assaulted and fettered his Intellectual Powers, broke in upon him, his Head should run much upon the Horror of being *Deceived at the last*. Yea, had there not been anything at all of a *Natural Debilitation* and *Obnubilation* in it, yet it were a very *Supposeable* thing, and not at all to be wondered at, if the *Serpent* be let loose to vex a *Servant of GOD* in the *Heel* of his *Life*; and if the *Powers of Darkness*, knowing the *Time to be short*, fall with *Great Wrath* on the Great Opposers of their *Kingdom*, and make a very *Dark Time* for them just before the *Break* of the *Eternal Day* upon them. And how justly might it awaken the rest of us to *Work out our own Salvation with Fear and Trembling*, when we see such a Man as *Dr. Mather*, concerned with so much *Fear and Trembling*, lest he should be *Deceived at the last*? . . . The best Judges of Things have agreed in this Judgment; That going to Heaven in the way of *Repentance*, is much safer and surer than going in the way of *Extasy*.³⁵

This passage not only illustrates the difference between Increase's earlier pronouncements and his actual deathbed behavior; with equal force it

³³ Miller, *The New England Mind*, 37-38; Ludwig, *Graven Images*, 108.

³⁴ Increase Mather, *Several Sermons* (Boston, 1715), 59-60.

³⁵ Cotton Mather, *Parentator*, 207-08.

points out the dissonant nature of the father's experience of death and the son's "rhetorical" interpretation of it. Cotton, after all, remained clearly convinced of his father's salvation, despite the force of his father's despair.

Increase Mather had an exceptional recording secretary in his son, and such meticulous descriptive passages are more the exception than the rule among extant materials. Still, the elder Mather's experience was not unique, nor was it contrary to what was expected of devout Puritans. More than forty years before Increase Mather's birth, William Perkins had opened one of his works with a quote from Ecclesiastes—"The day of death is better than the day that one is borne"—and then proceeded to observe that "not only wicked and loose persons despaire in death, but also repentant sinners, who oftentimes in their sickness, testifie of themselves that being alive and lying in their beds, they feele themselves as it were to be in hell, and to apprehend the very pangs and torments therof."³⁶

One of the earliest extant funeral sermons preached in New England was that of Samuel Wakeman for the departed soul of John Tappin of Boston, a victim of death at the age of eighteen. That Tappin was a godly youth is testified to by Wakeman and appears evident from the entire middle section of the sermon, a warning to the "rising generation" to make haste in "setting their hearts seriously God-ward," which was written by the young man for delivery upon his death. Still, when in the end Wakeman describes Tappin's final moments, he notes that Tappin

looked upon himself an undone man without an interest in Jesus Christ; yet he was not without some hope that he was at peace with God in him, yet not without fears, bemoaning himself in respect of his hardness of heart and blindness of minde, and that he had been no more thoroughly wrought upon by the Means that he had formerly enjoyed. O Sirs, Dying times are Trying times.³⁷

Writing seven years later, Leonard Hoar sounds almost jealous of what he sees as the comparative ease with which too many of the ungodly depart the world:

I acknowledge its an error in the saints and people of God to be so much affrighted at death, and to goe so mournfully out of this world: surely they have not learn'd to look off this world, and to look up to that which is to come; yet I am sure it is a greater error, and a damning error in the wicked and ungodly when they are not afraid of Death at all, when they look upon Death as a common and usual event and have slight thoughts of it, all their complaints are out of sympathy with their friends, or from their bodily pains, or their distracted thoughts about worldly matters, but never consider what a weighty thing it is to dye well, or what a dreadful thing it is to miscarry in their latter end. And O the wretched stupidity of some that can be then most secure, and are even like the beasts which perish, *Psal.* 49.12. O then blame not any that have woeful apprehensions of death, and beware of indulging your selves in a stupid secure

³⁶ Perkins, *Salve for a Sicke Man*, 6.

³⁷ Samuel Wakeman, *A Young Man's Legacy to the Rising Generation* (Boston, 1673), 45.

frame, for as Solomon saith in another case, *Prov.* 23.32. It will sting like an Adder and bite like a Serpent.³⁸

As might be expected, Cotton Mather also had something to say on the subject. "I have seen Persons Quaking on their Death beds," he writes, "and their very Beds therewith Shaking under them; From whence their first Shriek unto me, has been, *O! Sir, The wrath of a Dreadful God, makes me Tremble; I Tremble, I Tremble, at that Wrath.*" Further, he notes:

I knew, a very stout man, who, as he was going to Dy, said, *I have been among Drawn Swords, and Armed men; I have stood before the mouths of Roaring Canons, and where the Bullets have come as Thick as Hail about me; and yet I never knew what it was to be afraid: But now I am apprehensive of my being Exposed unto the Anger of the Almighty God, my Heart is overwhelmed with the Dread of it, my Heart even Dies within me, at the Thought of That!*³⁹

And then there is the testimony of the tombstone. The epitaph on the James Hickox stone reads:

Great God, how oft thy wraith appears
And cuts off our expected years
Thy wraith awakes our humble dread
We fear the Tyrant which strikes us dead.⁴⁰

The reason for this apparent dread of death among godly Puritans—"a King of Terrors," as it was often called⁴¹—seems rooted in a combination of their theology and their everyday sense of reality. Their theology taught them of their utter and total depravity, of their helplessness in securing their own salvation, and reinforced this pessimism with the doctrine of assurance; as noted earlier, doubt of salvation was essential to salvation and that Puritan who, for so long as he breathed, became at any time secure and comfortable in the knowledge of his salvation, was surely lost. A striking example of the kind of emotional stress created by this doctrine is the case of the deathbed scene of that formidable personality, John Knox, as related by William Perkins:

He [Knox] lay on his death bedde silent for the space of foure hours, very often giving great sighes, sobbes, and grones, so as the standers by well perceived that he was troubled with some grievous temptation: and when at length he was raised in his bedde, they asked him how he did, to whome he answered thus: that in his life he had indured many combates and conflicts with Satan, but that now most mightily the roaring lyon had assaulted him: often (said he) before he set my sinnes before mine eyes, often he urged me to desperation, often

³⁸ Hoar, *Sting of Death*, 11–12.

³⁹ Cotton Mather, *Thoughts of a Dying Man*, 37, 35–36.

⁴⁰ Ludwig, *Graven Images*, 82.

⁴¹ See, for example, Samuel Willard, *A Compleat Body of Divinity* (Boston, 1726), 234 (from Sermon 66, Oct. 31, 1693); and Cotton Mather, *Euthanasia* (Boston, 1723), 7.



Fig. 2. Triumph over the temptation of despair. Fifteenth-century block print, from *Ars Moriendi*.



Fig. 3. Detail of the Susanna Jayne gravestone, 1776, Marblehead, Massachusetts. Death, crowned with the laurel of victory, holds the sun and moon in either hand. Angels of heaven and bats of hell adorn each corner; the hooped serpent, symbol of eternity, surrounds Death. Photograph from Allan I. Ludwig, *Graven Images: New England Stonecarving and Its Symbols, 1650-1815* (Middletown, Conn.: Wesleyan University Press, 1966).



Fig. 4. Detail of the Joseph Tapping gravestone, 1678, King's Chapel, Boston, Massachusetts. An allegorical rendering of the fateful interplay between Time and Death. Photograph from Ludwig, *Graven Images*.

he laboured to intangle me with the delights of the world, but beeing vanquished by the sword of the spirit, which is the word of God, he could not prevaile. But now he assaults me another way: for the wily serpent would perswade me that I shall merit eternall life for my fidelitie in my ministrie. But blessed be God which brought to my minde such Scriptures whereby I might quench the fierie darts of the devill, which were, *What hast thou that thou hast not received:* and, *By the grace of God, I am that I am:* and, *not I but the grace of God in me:* and thus being vanquished he departed.⁴²

A break from the established Christian tradition of dealing with death can be clearly perceived here. In the *Ars Moriendi*, for example, the dying man is assaulted by a variety of demons tempting him with infidelity, despair, impatience, vainglory, and avarice, but the victim, aided by a battery of saints and angels, attains salvation by resolutely clinging to his optimistic belief in his own goodness and the justness of God.⁴³ Knox, on the other hand, though subject to similar temptations during his life, was tempted in precisely the reverse manner when death grew near—tempted, that is, by security founded on the heretical premise of good works—and attained salvation by denying it.

In order for the doctrine of assurance to be effective, it was essential that believers have an unquestioning faith in the reality of the contrasting terrors and bliss of the afterlife. At a time when, as D. P. Walker has pointed out, the doctrine of eternal torment for the damned was beginning to come under attack in England and on the Continent, among the New England Puritans there remained no plainer reality.⁴⁴ And while it is true that in the past the fire and brimstone nature of the Puritan sermon and tract has been greatly exaggerated, it is equally true that the New England ministry did not hesitate to conjure up explicit pictures of the terrors of hell when it suited them, with the result that, in the words of Increase Mather's uncommon understatement, "oft times there were more weepers than sleepers in the Congregation."⁴⁵ Thus, "If their Strength were the Strength of Stones," Solomon Stoddard wrote of the unregenerate in 1713,

or their Flesh of Brass, they could not endure their Misery. They will have *Anguish of Spirit*, not know what in the World to do; there will be dreadful Wailing, Mat. 13.42. They will lament their Sins, they will bewail the loss of Opportunities; they will condemn their Folly, they will curse themselves, they will wish they had never seen such things as now their Hearts dote upon . . . they will wish they had no Senses; their Hearing and Seeing and Feeling will be their Misery, their Memory, their Understanding, their Conscience will be their Torment; they will wish they had no Bodies, and wish they had no Souls, their *Bodies and Souls will be Vessels of Wrath*.

⁴² Perkins, *Salve for a Sicke Man*, 55.

⁴³ *The Ars Moriendi* ([ca. 1450]; reproduction, London, 1881).

⁴⁴ D. P. Walker, *The Decline of Hell* (Chicago, 1964).

⁴⁵ In Mitchel, *Discourse of the Glory*, viii. Mather is here specifically referring to Mitchel's ministry.

Further, he reminds his audience, all the sufferings of this place where "the Worm dyeth not," where "the Fire is not quenched," are eternal:

The duration of their Misery cannot be measured: We may measure the breadth of the Earth, and the circuit of the Heaven, but can't measure Eternity. Add thousands to thousands, and multiply Millions by Millions; fill Quires of Paper with numbers, and you can't measure Eternity; It cannot be divided into Days, or Years, or Ages; make never so many Parts of it, one will be Eternal: When Men have suffered never so long, there is an Eternity remaining: it don't grow shorter and shorter. This makes every part of their Misery Infinite, their pain will be Infinite, the Terroure Infinite. If Miseries End, there is an opportunity for Comfort afterwards; but Eternity cuts off opportunities for Comfort: Men may well say, *Who can dwell with everlasting Burnings?*⁴⁶

In the middle of the seventeenth century the Anglican bishop, Jeremy Taylor, had assured his readers that "God knows that the torments of hell are so horrid, so insupportable a calamity that He is not easy and apt to cast those souls which he hath taken so much care and hath been at so much expense to save, into the eternal, never-dying flames of hell lightly, for smaller sins, or after a fairly begun repentance, and in the midst of holy desires to finish it." And a century later Marie Huber was writing tracts with a wide European influence, arguing that "the Doctrine of the Eternity of Hell-Torments was not so incontestable, as not to be called in question by a great number of judicious men."⁴⁷ But at the same time Jonathan Edwards was traveling about New England, drawing and spellbinding crowds with such passages as:

How dismal will it be, when you are under these racking torments, to know assuredly that you never, never shall be delivered from them; to have no hope: when you shall wish that you might but be turned into nothing, but shall have no hope of it; when you shall wish that you might be turned into a toad or a serpent, but shall have no hope of it; when you would rejoice, if you might but have any relief, after you shall have endured these torments millions of ages, but shall have no hope of it; when after you shall have worn out the age of the sun, moon, and stars, in your dolorous groans and lamentations, without any rest day or night, or one minute's ease, yet you shall have no hope of ever being delivered; when after you shall have worn out a thousand more such ages, yet you shall have no hope, but shall know that you are not one whit nearer to the end of your torments; but that still there are the same groans, the same shrieks, the same doleful cries, incessantly to be made by you, and that the smoke of your torment shall still ascent up forever and ever; and that your souls, which shall have been agitated with the wrath of God all this while, yet will still exist to bear more wrath; your bodies, which shall have been burning and roasting all this while in these glowing flames, yet shall not have been consumed, but will remain

⁴⁶ Solomon Stoddard, *The Efficacy of the Fear of Hell to Restrain Men From Sin* (Boston, 1713), 24, 26.

⁴⁷ Taylor, *Holy Dying*, 85; Marie Huber, *The World Unmask'd* (London, 1736), 262.



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Fig. 6. Detail of the left panel of the William Dickson gravestone, 1692, Cambridge, Massachusetts. This imp of the underworld carries an hourglass and is armed with the arrow of Death. Photograph from Ludwig, *Graven Images*.



Fig. 7. Detail of the right panel of the Rebekah Bunker gravestone, 1709, Cambridge, Massachusetts. An example of the erotic imagery that sometimes is found in Puritan gravestones. Photograph from Ludwig, *Graven Images*.

Fig. 5, facing page. Detail of the Thomas Kendel gravestone, ca. 1678, Wakefield, Massachusetts. Winged death-head supported by a pillar probably signifies the inevitable triumph of death. Photograph from Ludwig, *Graven Images*.

to roast through an eternity yet, which will not have been at all shortened by what shall have been past.⁴⁸

Even Charles Chauncy, Edwards's determined Puritan rival and a well-known religious "liberal," was not outside the mainstream. As he warned his congregation in the summer of 1741:

There is nothing betwixt you and the place of blackness of darkness, but a poor frail, uncertain life. You hang, as it were, over the bottomless pit, by the slender thread of life, and the moment that snaps asunder, you sink down into perdition. . . . Who has bewitched you, O sinners, that you are thus lost to all sense of your own safety and interest! Be convinc'd of your danger. You are certainly in a state of dreadful and amazing hazard.⁴⁹

As death drew near for the Puritan the tension normally built into the doctrine of assurance became increasingly more intense, for now the time of decision was at hand, the time when the Puritan's sin-riddled soul would be judged and either admitted to heaven or cast into the fiery pit of hell. And if he or she was not properly prepared there was no way out, no such thing, as the Catholics and some early Christians believed, as a last-minute sacramental reprieve. Whereas centuries earlier the dying Empress Matilda, mother of the future Henry II, had supposedly "distributed her treasures to widows, orphans, and the poor and so escaped the peril of death," to the Puritan such deathbed gestures of repentance were of little value.⁵⁰ "There is no Real Conversion in it," Cotton Mather argued, "Men are then only like Iron softened in the Fire; they soon Return to their former Hardness if God spare them from going down into the *Unquenchable* Fire."⁵¹ At death the Puritan knew there was nothing he could do but wait, hope—and doubt.

It is hardly surprising, then, to read James Fitch, in the earliest New England funeral sermon we have, explaining the Puritan's frequent lack of "the sweetness of that unspeakable peace in his dying hour"—the inherited prescription for Christian deathbed behavior—as the result of the fear of "the misery of falling short, for none can be so sensible of that, as those who know experimentally the preciousness of Christ and heavenly things: and though also many times the very thought, what if deceived? what if fall short at last? that thought would make the flesh tremble."⁵² Or, to cite once again the vivid language of Leonard Hoar, who after recognizing the historical tradition with such typical phrases as "the day of ones Death is better than the day of ones Birth," and rhetorically describing death as "a loosing from

⁴⁸ Jonathan Edwards, "The Eternity of Hell Torments," Sermon 11 in Edwards, *Works*, 4 (New York, 1843), 278.

⁴⁹ Charles Chauncy, *The New Creature Describ'd* (Boston, 1741), 20. For a thorough, if somewhat more pedestrian, summing up of the Puritan view of hell, see John Bunyan, *Sighs From Hell or Groans of a Damned Soul* (Boston, 1708).

⁵⁰ Quoted in Boase, *Death in the Middle Ages*, 124.

⁵¹ Cotton Mather, *Thoughts of a Dying Man*, 40–41.

⁵² James Fitch, *Peace the End of the Perfect and Upright* (Boston, 1672), 6.

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this troublesome Shore," becomes much more specific, much truer to the Puritan vision:

So it may be said of every inhabitant of this earth when he comes to dye, the weight of sin, the unsupportableness of Gods anger, the terrors of hell, the nearness of the danger, the difficulty of salvation will all appear nakedly to the naked soul. When God makes darkness and it is night, then the beasts of the forest creep forth: every frog will be croaking towards the evening, every puddle will send up a stinking vapor in a foggy night; all the several shadows of things will unite, every [illegible] will concur to make up and compleat the misery of the poor sinner. . . . *Hence learn why men dread and are so afraid of death*, yes there are many causes why a natural man should fear Death, because of the sting that is in it.⁵³

The acute awareness that man is both powerless to affect the matter of his salvation and morally crippled by his natural depravity caused, Cotton Mather writes, much distress and retrospective agonizing as death closed in. "Tis very certain," he notes in *Thoughts of a Dying Man*, "that at the Last, when you are taking your leave of this World, you will be full of Disdainful Expressions concerning it, and Express your selves to this purpose: *Vain World! False World! Oh! that I had minded this World Less, and my own Soul more, than I have done!*" Later in the same work Mather warns that when, on your death bed, you are provided with reminders of your past sinful life, "the sight of them will smite thee with more Horror, than if so many *Rattle Snakes* were then horribly crawling about thee." And still later:

Tis no rare Thing, for eminent *Saints*, when they lay a *Dying*, to profess, as we find in the History of their *Lives*, that some of them have done; *The Loss of Time, is a Thing, that now Sits heavy on this Poor Soul of mine!* . . . Men ordinarily Dy, with words, like those of that Great Person, Sir Henry Wotten, uttered with Tears, *How much Time have I to Repent of! and how Little Time to do it in!*⁵⁴

The New England Puritans, despite their traditional, optimistic rhetoric, were possessed of an intense, overt fear of death—and for three very good and very rational reasons: their belief in their own utter and unalterable depravity, in the omnipotence and justness of God, and in the unspeakable terrors of hell. Unlike Bacon, they did not fear death merely "as children fear to go in the dark"; they feared it because they knew precisely what to expect from it. One cannot leaf through Increase Mather's *Remarkable Providences*, or Cotton Mather's *Wonders of the Invisible World*, or even the relatively pragmatic John Winthrop's *Journal*—in which, for example, he relates in great detail the birth of a monster to a heretic as an example of

⁵³ Hoar, *Sting of Death*, 10–11.

⁵⁴ Cotton Mather, *Thoughts of a Dying Man*, 9, 15–16, 27–28.

God's "instruction of the parents"⁵⁵—without sensing the sober reality of the spiritual world to the mind of the devout Puritan. There was thus nothing extraordinary in their emotionally turbulent reaction to death; rather, given the essence of their belief, it was only the extraordinary, and possibly suspect, Saint who could face death calmly.

"THE POWER OF RELIGION depends, in the last resort," Peter L. Berger writes, "upon the credibility of the banners it puts in the hands of men as they stand before death, or more accurately, as they walk, inevitably, toward it."⁵⁶ If the Puritans experienced what may seem inordinate difficulty in facing death, it was not merely because of the fearful images of irresistible destiny conjured up in their collective mind's eye; nor, certainly, can the answer be found in the continued presence of the rhetoric of Christian optimism, of the *Migratio ad Dominum* theme, in their formal thoughts on the matter. The difficulty lay rather in reconciling these two contradictory "banners," which they carried simultaneously. As Berger's observation suggests, the discomfort they experienced was principally due to a lack of credibility in the answers they gave to questions concerning the proper way of viewing death and of experiencing dying. By clinging to the rhetorical tone and style (the ritual) of a Christianity equipped with a variety of mechanisms whereby man might affect his fate and secure his own salvation, a Christianity that had little application to their own deterministic concept of reality, the Puritans trapped themselves between conflicting belief systems—more specifically, in this case, between conflicting schemes of ethos and world view.

The functional effects of such conflicts have long interested theorists in the behavioral sciences. The concept of *anomie* or "normlessness," for example, has had a profound effect on modern sociology and psychology.⁵⁷ Although the subject of a great deal of controversy, the term in its simplest sense has generally been recognized as referring to the confusion and apprehension that may result from "a clash between belief-systems or, more precisely, a conflict between the *directives* of belief-systems."⁵⁸ Similarly, Leon Festinger's "cognitive dissonance" theory rests on the assumption that "the human organism tries to establish internal harmony, consistency, or congruity" in its cognitive structure; the loss of such consistency, Festinger argues, results in a state of psychological discomfort (or cognitive dissonance), the

⁵⁵ John Winthrop, *Journal* (New York, 1908), 1: 267–68.

⁵⁶ Peter L. Berger, *The Sacred Canopy: Elements of a Sociological Theory of Religion* (Garden City, N.Y., 1969), 51.

⁵⁷ For a concise discussion of both the sociological and psychological uses of the term, see Robert K. Merton, *Social Theory and Social Structure* (3d ed., enlarged; New York, 1968), 215–18.

⁵⁸ Sebastian De Grazia, *The Political Community: A Study of Anomie* (Chicago, 1948), 72.

intensity of which varies in relation to the importance of the incompatible and competing cognitions.⁵⁹

It is not my intent here to try to pin either of these or any other such specific labels on the Puritans' handling of the problems of death and dying. Such an exercise would properly be subject to both philosophical and substantive criticisms that are beyond the scope of this article. Nevertheless, what appears to be at work here is a phenomenon so basic that it transcends disciplinary and chronological boundaries. Whatever label we may care to give it, virtually all cultures and individuals experience an almost constant, if generally slight, changing of structure or focus as adjustments are made to cognitive and conceptual conflicts. On those occasions when such accommodation does not take place, particularly in relatively closed cultures when the matter at hand is of primary cultural significance, the reverberations of the continuing conflict may be felt in the very roots of the body in question.

Death and dying were matters of critical importance to the Puritans; indeed they constantly urged themselves to direct their lives toward that moment when their earthly pilgrimage would end. At the same time, however, their concept of death and its attendant and deserved terrors was at odds with their inherited Christian advocacy of displaying what James Fitch called "the sweetness of that unspeakable peace" when death approached. The result was a kind of cultural dissonance, an uncomfortable tension, that pressed for resolution. A culture, no less than an individual, cannot long endure such pressure. One of the principal ways of reducing such tension, Festinger and others agree, is "by changing one or more of the elements involved in dissonant relations,"⁶⁰ and this, it now seems clear, is what happened in New England in the eighteenth century.

Precisely when such a change may have taken place remains a matter of conjecture. Indeed, as with most important changes in the structure of intellectual and cultural phenomena, it may well be impossible ever exactly to locate a watershed point. But by the latter half of the eighteenth century, when the emerging New England orthodoxy was of an increasingly liberal bent, there is little evidence that the anxiety-riddled tension between death and dying that so beset devout Puritans a century earlier remained an active force.

This is certainly not to suggest that the Puritans' successors ceased to fear death. They did fear it, and still do; but in ways very different from the Puritans.⁶¹ Just as Christians in the Middle Ages and earlier fitted their fear

⁵⁹ Leon Festinger, *A Theory of Cognitive Dissonance* (Evanston, 1957), 260.

⁶⁰ *Ibid.*, 264.

⁶¹ For a concise summary of recent thought on the contemporary relationship between death and religion, see Irving E. Alexander and Arthur M. Adlerstein, "Death and Religion," in Feifel, *Meaning of Death*, 271-83. Cf. many of the articles in Hendrik M. Ruitenbeck, ed., *Death: Interpretations* (New York, 1969), and a recent sweeping criticism of most such studies to date, Barbara Chasin, "Neglected Variables in the Study of Death Attitudes," *Sociological Quarterly*, 12 (1971): 107-13.

into a fairly harmonious system of belief, so have most post-Puritan Christians, with the result that the fear is at least tempered by theological rationalization which is tolerably consistent. The Puritans inherited part of that rationalization, the part that counsels a peaceful death for the regenerate, and it recurs like an unthinking, though obviously important slogan throughout their writing; but it was not consistent with their sense of reality. It did not "fit." And so, although the need for such comforting counsel was apparently present, the reassurance it should have afforded the regenerate individual was denied, both by the determinism of the faith and by the excruciatingly difficult balance that had to be maintained between assurance and security. To return to Peter Berger's imagery, there was little real confidence possible in the credibility of the religious banners the Puritans carried as they walked inevitably toward death. The result was a vision of death and an attitude toward dying that were locked in perpetual conflict, a conflict that brought extraordinary discomfort to bear on the life of the devout Puritan, a conflict that could not be indefinitely endured.

The *Encyclopédie* Wars of Prerevolutionary France

ROBERT DARNTON

THE PUBLICATION OF THE *Encyclopédie* has long been recognized as a turning point of the Enlightenment. In permitting Diderot's text to appear in print the state, however reluctantly and imperfectly, gave the philosophes an opportunity to try their wares in the market place of ideas. But what was the result of this break-through in the traditional restraints on the printed word in France? By concentrating on the duel between the *encyclo-pédistes* and the French authorities, scholars have told only half the story. The other half concerns some basic questions in the social history of ideas: how did publishers plan and execute editions in the eighteenth century? How well did works like the *Encyclopédie* sell? And who bought them? This essay is addressed to those questions. By recounting the life cycle of one book, it is intended to suggest some of the possibilities in the history of publishing, a field that has lain fallow too long despite its attractive location at the crossroads of intellectual, social, economic, and political history.¹

¹ This essay, which is intended as a preliminary sketch for a full-length study of the quarto editions of the *Encyclopédie*, is based almost entirely on the papers of the Société Typographique de Neuchâtel (hereafter STN) in the Bibliothèque de la Ville de Neuchâtel, Switzerland. All citations are to those manuscripts unless specified otherwise. Any researcher concerned with the later editions of the *Encyclopédie* is bound to feel indebted to the painstaking scholarship of two men: George B. Watts and John Lough. See especially Watts's articles, "Forgotten Folio Editions of the *Encyclopédie*," *French Review*, 27 (1953-54): 22-29, 243-44; "The Swiss Editions of the *Encyclopédie*," *Harvard Library Bulletin*, 9 (1955): 213-35; and "The Genevan Folio Reprinting of the *Encyclopédie*," *Proceedings of the American Philosophical Society*, 105 (1961): 361-67; and see Lough's book, *Essays on the Encyclopédie of Diderot and D'Alembert* (London, 1968). As far as the circulation of books within France is concerned the first edition of the *Encyclopédie* was relatively unimportant, but it has attracted most of the attention of scholars because its publication became the crucial episode in the liberalization of the Direction de la Librairie and in the battles between the philosophes and their opponents during the 1750s. A decree of the king's council suppressed the first two volumes of the *Encyclopédie* in 1752; and the council revoked the privilege for the book in 1759, when it had come under attack by the pope, the Jesuits, the Jansenists, the Parlement of Paris, and other enemies of the philosophes. But C. G. de Lamoignon de Malesherbes, the enlightened director of the Librairie, unofficially permitted the last ten volumes of text to appear in 1765. The last two volumes of plates were published in 1772. For an excellent synthesis of the scholarship on this aspect of the history of the *Encyclopédie*, see Arthur M. Wilson, *Diderot* (New York, 1957, 1972), and Jacques Proust, *Diderot et l'Encyclopédie* (Paris, 1967).

WHEN DIDEROT AND HIS PUBLISHERS brought out the last volume of the *Encyclopédie* in 1772, they had won more than a moral victory over the system for controlling French publishing. The first edition probably produced about 2,500,000 livres in gross profits. But the government refused to let the book sell openly, and most of the 4,225 sets went to customers outside France.² The second edition also seems to have been primarily a non-French affair. It was a folio reprint of the original text, produced in Geneva by a consortium of publishers allied with Charles Joseph Panckoucke of Paris. Its sales records have not survived, but its publishers originally hoped to market half of their 2,200 sets in France; and they had sold 1,330 sets throughout Europe when they settled their accounts in June 1775.³ So by that date only 3,000 copies of the first two editions, at the very most, existed in France. The country had not been inundated with *Encyclopédies*, despite the semi-legal status granted to the book.

But the publishing of the next editions—the three quarto and the two octavo printings of the original text—is a very different story; and unlike the publishing history of the first two editions, it can be told in detail, thanks to the papers of the Société Typographique de Neuchâtel in Neuchâtel, Switzerland. The story begins with Panckoucke, the extraordinary entrepreneur known as “the Atlas of the book trade,”⁴ and his system of alliances and alignments within the world of publishing and politics.

In December 1768 Panckoucke bought from the original publishers the plates of the *Encyclopédie* and the rights to future editions of it. Precisely what these rights were is difficult to say. Panckoucke used the terms “droits” and “privilege” throughout his correspondence, but the government had revoked the formal privilege of the *Encyclopédie* in 1759, and the registers of privileges in the Bibliothèque Nationale give no indication that it was ever restored. They do reveal that Panckoucke received a twelve-year *privilege général* on March 29, 1776, for a “Recueil des planches sur les sciences, arts et métiers,” which may have been enough to substantiate his claim to possess a kind of copyright.⁵ In any case, he asserted that claim in the most abso-

² On the economic aspects of the first edition, see, in addition to the works cited above, Norman L. Torrey, “L’*Encyclopédie* de Diderot, une grande aventure dans le domaine de l’édition,” *Revue d’histoire littéraire de la France*, 51 (1951): 306–17; John Lough, “Luneau de Boisjermmain v. the Publishers of the *Encyclopédie*,” *Studies on Voltaire and the Eighteenth Century*, 23 (1963): 115–73; and Ralph H. Bowen, “The *Encyclopédie* As a Business Venture” in Charles K. Warner, ed., *From the Ancien Régime to the Popular Front: Essays in the History of Modern France in Honor of Shepard B. Clough* (New York, 1969), 1–22. What little is known about the sales of the first edition comes from the papers of Luneau de Boisjermmain, which are too polemical to be trustworthy and which justify estimates of the foreign sales ranging from somewhat more than one-half to three-quarters of the edition.

³ Lough, *Essays on the Encyclopédie*, 103.

⁴ George B. Watts, “Charles Joseph Panckoucke, ‘L’Atlas de la librairie française,’” *Studies on Voltaire and the Eighteenth Century*, 68 (1969): 67–205.

⁵ *Privilege* no. 613, Mar. 29, 1776, Bibliothèque Nationale, fonds français, MS 21967. On February 10, 1776, Panckoucke had received a twelve-year *privilege général* (*privilege* no. 365, *ibid.*) for “un ouvrage qui a pour titre *Nouveau dictionnaire des arts et des sciences etc.*,” but nothing proves that this work was the *Encyclopédie* or had any connection with it. None of

lute manner, citing not only the contract by which he bought out the original publishers but also the sanction of the French government; and he sold portions of his “privilege” to a whole series of partners, periodically buying them back and reselling them again to new associates for new editions.

Panckoucke’s first *Encyclopédie* was the second edition, the folio reprint of 1771–76. Those were hard years in the book trade, owing to the repressive measures of the “triumvirate” ministry of Maupeou, Terray, and d’Aiguillon, so Panckoucke had the edition printed in Geneva by his partners, who included Voltaire’s publisher, “the angel Gabriel” Cramer. It was a stormy affair, involving quarrels among the associates, conflict with a rival, a “Protestant” *Encyclopédie* being produced by Barthélemy de Félice in Yverdon, and a losing battle with the French government, which had confiscated six thousand volumes that Panckoucke had originally printed in Paris. Whether Panckoucke ever had much success in cracking the French market with this edition cannot be known, but his difficulties did not discourage him. By the accession of Louis XVI he remained convinced that there was still a fortune to be made in *Encyclopédies*, and the liberal character of the new ministry swelled his hopes. He found doors opening for him everywhere within the government. His coach carried him into Versailles “like an official with a portfolio.”⁶ And his letters burgeoned with assurances of “protections” from lieutenants of police, directors of the book trade, and ministers.

On July 3, 1776, Panckoucke sold an interest of fifty per cent in his newly consolidated “rights and privileges” in the *Encyclopédie* for 143,000 livres to the Société Typographique de Neuchâtel, one of the most important publishers of French books during the twenty years before the Revolution. After toying with a plan to publish another folio reprint, this new association decided to produce a completely revised edition. The text was to be rewritten by a whole stable of philosophes—including Marmontel, Morellet, La Harpe, D’Arnaud, St. Lambert, and Thomas—under the direction of Suard, with D’Alembert and Condorcet as associates. Panckoucke did not enlist Diderot, “une mauvaise tête, who demanded 100,000 écus and would have driven us to despair.”⁷ But he counted heavily on D’Alembert, who was to solicit the protection of Frederick II and perhaps even to persuade him to accept the dedication of the new work. D’Alembert also considered writing a history of the *Encyclopédie* for the new edition, but that essay died

the other registers of *privileges* or *permissions tacites* contain any references to the *Encyclopédie* for the period 1768–88: see MSS 21964–67, 21983, 21984, 21989, 22000–02, 22013, and 22073. On September 8, 1759, after the revocation of the original privilege of the *Encyclopédie*, its first publishers received a privilege to produce a collection of its plates, which they probably construed as a “copyright” for the entire work and sold to Panckoucke.

⁶ D.-J. Garat, *Mémoires historiques sur la vie de M. Suard, sur ses écrits et sur le XVIII^e siècle* (Paris, 1820), 1: 274. For more details about the second edition, see Lough, *Essays on the Encyclopédie*, 52–111.

⁷ Panckoucke to STN, Aug. 4, 1776. In this letter Panckoucke explained that he was referring to an interview with Diderot that had occurred eight years earlier. For Diderot’s version of this famous encounter, see Wilson, *Diderot*, 578–79.

stillborn, like other potential classics of the Enlightenment—a history of French Protestantism by Raynal, a history of Turgot's ministry by Voltaire—that never got beyond the stage of projects knocked about in negotiations between authors and publishers. In the end this new *Encyclopédie* itself miscarried, despite the grandiose plans of its backers, because it was undercut by a quarto edition of the original text, which was launched in 1776 by Joseph Duplain of Lyons, the antihero of this story and one of the most intrepid buccaneers in the era of “booty capitalism.”

Like many provincial bookdealers Duplain built his business on the demand for cheap, pirated works, often of a racy or philosophical character, which were produced in the printing houses flourishing beyond the fringes of France's borders, thanks to the system of privileges and thought control that stifled innovative publishing within the kingdom. Duplain smelled a fortune in cut-rate *Encyclopédies*. He announced the opening of a subscription for a cheap quarto edition, which would incorporate the five-volume supplement in the original text. He protected himself by attributing the edition to Jean Léonard Pellet, a Genevan printer who received three thousand livres for acting as straw man. And when the flow of subscriptions proved strong enough, Duplain contracted the printing to several Genevan shops, keeping the financial and administrative work to himself. He counted on getting the books into France either by smuggling—he had great influence in the booksellers' guild of Lyons, although he had powerful enemies in the Parisian guild—or by winning the benevolent neutrality of the French authorities. But he had not reckoned with Panckoucke.

Panckoucke could choose either to beat Duplain or to join him. The first alternative appealed to Panckoucke because he was convinced that he could use his protections effectively enough to block the channels of the underground book trade. But the success of the subscription created a greater temptation. Panckoucke knew “every step that Duplain takes,”⁸ thanks to secret reports from an allied Lyonnais bookseller called Gabriel Regnault. Regnault learned that the subscription was selling spectacularly, and corroborative information “from everywhere”⁹ made it look as though the quarto *Encyclopédie* could turn into the most profitable publication of the century. So Panckoucke shelved the project for the revised edition and entered into negotiations, bartering his monopoly on legality against a cut of the subscriptions. On January 14, 1777, he and Duplain signed what later became known as the “Treaty of Dijon.” Each took a half interest in the quarto enterprise, which they subsequently divided among their own associates (the Société Typographique de Neuchâtel eventually came to own five twenty-fourths of the entire enterprise). Duplain committed himself to administer the production, distribution, and financing of the edition accord-

⁸ Panckoucke to STN, Dec. 26, 1776.

⁹ Panckoucke to STN, May 13, 1777.

ing to conditions specified in great detail by the contract. And Panckoucke promised to supply half the capital, the three volumes of plates, and the covering protection of his privilege. The last item was no small advantage. In August 1777 Panckoucke wrote that Le Camus de Neville, the director of the Librairie, “will protect our great affair” and had even given permission for Panckoucke to import the books directly to his warehouses in Paris, bypassing the customs, the booksellers’ guild, and the censorship.¹⁰ At the same time, writing as if he were himself a minister, Panckoucke directed the inspector of books in Lyons to give clear passage to the crates being shipped from Switzerland.¹¹ In fact Panckoucke pulled strings so effectively that the Swiss printers began to stuff their shipments of *Encyclopédies* with prohibited books. Far from drawing the fire of the established authorities, as it had done in the 1750s, the *Encyclopédie* circulated under the protective covering of their patronage; and that protection served as camouflage for the diffusion of works that the state wanted to suppress.

Panckoucke and Duplain had no idea that a small smuggling operation had grafted itself onto their enterprise. They gave all their attention to the maximization of profits, and the quarto proved to be extraordinarily profitable: orders poured in from everywhere, traveling salesmen reaped unheard-of harvests, and booksellers marveled at a hunger for the *Encyclopédie* that had remained dormant among clients who had not been able to buy the folio editions. “There is no other work so universally widespread,” wrote Dufour of Maestricht. “Our streets are paved with it,” said Resplandy of Toulouse, echoing exactly the observation of a Lyonnais salesman: “Our town is paved with it.” And Panckoucke exulted, “The success of this quarto edition passes all belief.”¹² In opening the subscription Duplain had set his sights high: he hoped to sell 4,000 copies. The subscription filled to overflowing with astonishing speed; so Duplain opened another, for 2,000 more copies. It, too, filled rapidly, and Duplain opened a third, making a total of 8,000 sets of thirty-nine quarto volumes each—an extraordinary amount for an era when printings of single-volume works normally ran to 1,000 copies or so.

This succession of subscriptions explains the mystery of the missing second quarto edition, which has plagued bibliographers who have been able to locate only the first, or “Pellet,” edition and the third, or “Neuchâtel,” edition of the quarto *Encyclopédie*.¹³ Duplain committed himself to print

¹⁰ Panckoucke to STN, Aug. 5, 1777.

¹¹ “Je vous serai obligé de donner vos ordres pour que ces volumes passent sans difficulté et d'accorder toute votre protection à cet ouvrage. M. de Neville est prévenu de tout ce que j'ai fait à ce sujet.” Panckoucke to La Tourette, July 18, 1777, Bibliothèque publique et universitaire de Genève, MS supp. 148.

¹² Dufour to STN, Aug. 2, 1780; Resplandy to STN, Jan. 2, 1778; D'Arnal to STN, Nov. 12, 1779; Panckoucke to STN, Sept. 9, 1777.

¹³ George B. Watts made a good guess as to the explanation of the “second” edition, but he mistakenly believed that Pellet directed the whole affair: see “Swiss Editions of the *Encyclo-*

the second subscription when the printers had reached sheet "T" of volume 6, working at a press run of 4,000 copies. He directed them to reprint 2,000 copies of everything they had completed and then to continue at a run of 6,000. So there was no distinct second edition. The third subscription coincided with a separate "third edition," because each sheet was reset and run off at 2,000 copies, and the title page of each volume proclaimed it to be "troisième édition, à Neuchâtel, chez la Société typographique." In fact this imprint was a ruse devised by Duplain to inveigle subscriptions from persons who had been put off by the slipshod quality of the Pellet editions. The Société Typographique actually printed only one volume of "its" edition and four of the volumes that appeared under Pellet's name. In every case Duplain subcontracted the printing and remained hidden behind his typographical false fronts.

Duplain used printers in Neuchâtel, Geneva, Lyons, Trévoux, and Grenoble, putting more than forty presses at work to turn out about 300,000 volumes. To produce and distribute books on such a scale required assembling one of the largest operations in premodern printing and strained resources throughout the publishing industry. For two and a half years the *Encyclopédie* dominated printing in the region around Lyons. "Except for a few liturgical works, nothing else is being printed here, in all the shops, only the *Encyclopédie*," an agent reported in 1778.¹⁴ The Société Typographique took five months, using about half the capacity of its twelve presses and its work force of about thirty-five men, to print a press run of 6,000 copies of one of the huge, double-column tomes. Financing 8,000 copies of thirty-six such volumes required so much capital that Panckoucke and Duplain fell back on consortia of French and Swiss bankers, and the same agent in Lyons observed, "Whoever had a little money to put into books every month or every year has placed it on the *Encyclopédie* quarto."¹⁵ The *Encyclopédie* consumed so much paper that in December 1777 a buyer for the Société Typographique could not find a single sheet of the requisite kind in Lyons. The Société managed to continue printing only by sending paper scouts throughout France and western Switzerland in search of every last ream of *fin*, twenty-pound (Lyonnais measure) *carré* or *raisin*. Founders could not supply type rapidly enough to satisfy the demand (the quarto was printed, appropriately, in a type called "Philosophie"), and so some Genevan printers failed to begin work on schedule in 1777. The Neuchâtelois had to suspend printing at a crucial moment because they received a barrel of bad ink, and the inkmaker, a Parisian called Langlois who had a stranglehold on the quality-ink trade, kept inching up his prices, while lamenting

pédie," 228. Lough agrees with Watts's version of this bibliographical imbroglio: *Essays on the Encyclopédie*, 36-38.

¹⁴ Jean-François Favarger to STN, July 21, 1778.

¹⁵ *Ibid.*

about his own increased costs, which he attributed to poor olive harvests in the Midi. Wagoners also took advantage of increased orders to force up their rates. And the *Encyclopédie* produced chaos in the labor market of printing. Not only did the printers have to send hundreds of miles for workers, but the supply was so scarce that they took to raiding each other's shops through the use of industrial spies like Louis Marcinhes, a down-and-out watchmaker in Geneva, who wrote to the Société Typographique in July 1777,

Pellet and Bassompierre have by inflated promises seduced many workers and drained off the printing shops of the surrounding area. But they only want to pay them 15 florins 9 sols of our money per sheet. So a good number want to leave, because they are asking for 17 florins per sheet. The man leaving this week [for Neuchâtel] is one of those. He is called Caisle. Two pressmen, who have promised to come talk with me, also should leave. . . . I won't lose sight of any occasion to send to you the discontented from the shops of Pellet, Bassompierre, and Nouffer.¹⁶

In short, the quarto *Encyclopédie* sent repercussions into the remotest sectors of the economy. For it to come into being a whole world had to be set in motion: ragpickers, olive growers, financiers, and philosophers collaborated to create a work whose corporeal existence corresponded to its intellectual message. As a physical object and as a vehicle of thought, the *Encyclopédie* synthesized a thousand sciences, arts, and crafts; it represented the Enlightenment, body and soul.

Its publishers probably spent too much time calculating costs and profits to entertain such lofty thoughts. The Société Typographique estimated the total revenue of the enterprise at 2,454,092 livres, the total cost at 1,117,354 livres, and the gross profit at 1,336,738 livres: a return of one hundred twenty per cent on expenditures. No wonder they considered this affair "the most beautiful ever to be done in publishing,"¹⁷ or that it touched off a series of fierce commercial wars.

Duplain, who had originally floated the quarto as a privateering venture, had no way, once he turned legitimate, of burying his treasure. Other pirates got wind of it and raced to the attack. First came announcements of rival counterfeit editions from Geneva and Avignon. Panckoucke read them as bluffs and counseled his associates to ride them out, since "I have arranged everything here in such a manner that none of those editions can enter France, and without France no success."¹⁸ He was right: the announcements were a way of holding the quarto publishers up for ransom by threatening to undersell them unless they paid a certain sum in protection money.

¹⁶ Louis Marcinhes to STN, July 11, 1777.

¹⁷ STN to Panckoucke, Aug. 20, 1778. Because the accounts became extremely embroiled, it is impossible to know the exact costs and profit of the enterprise.

¹⁸ Panckoucke to STN, Sept. 9, 1777.

The danger in this game was that one could not distinguish between a fake and a real attack until he saw the whites of his assailant's eyes. After the quartos of Geneva and Avignon had disappeared over the horizon, J. S. Grabit and J. M. Barret of Lyons announced plans to publish another quarto *Encyclopédie*, and they proved that they meant business by actually printing a few volumes. In this case Duplain and Panckoucke agreed that it would be wiser to capitulate. They bought out Grabit and Barret for 27,000 livres—the rough equivalent of a lifetime's wages for one of their printers—and received in return only a legalized promise to abstain from further counterfeiting. Then they learned that a consortium of publishers in Lausanne and Bern planned to produce an even smaller, even cheaper *Encyclopédie*, an octavo edition that would sell for approximately 200 livres. This time Duplain and Panckoucke decided to stand and fight.

At first the quarto publishers hoped that the octavo venture would simply collapse. They joked that the small type of "cette miniature" would blind its readers, and Panckoucke proclaimed "that octavo edition may cause some alarm, but it won't hurt us. . . . It is folly to print the *Encyclopédie* in such a small text. Moreover, we will be defended here. I am waiting for the magistrate [Le Camus de Neville] to return so that I can reveal everything to him. I promise you firmly that that *Encyclopédie* will never enter France." The Société Typographique replied, "You hold the keys to the kingdom."¹⁹ But reports from provincial booksellers indicated that the octavo subscriptions were selling as spectacularly as the quarto had done. So the quarto group began pourparlers—not with any serious intention of making peace but rather to delay the execution of the octavo until the quarto could be completed and the new, revised edition announced, thereby stealing the octavo market. The publishers of Lausanne and Bern, who were veterans of pirate publishing, detected this strategy after a few rounds of negotiation and resolved to proceed with their printing. Duplain then attempted to overwhelm them with a frontal assault: he published an announcement that the quarto group would produce its own octavo edition at an even cheaper price than the octavo of Lausanne and Bern. On November 1, 1777, Lausanne and Bern retaliated with an ultimatum: withdraw your announcement within fifteen days, or we will drop the price of our octavo to the level of yours, and we will undermine your quarto by producing a still cheaper quarto of our own.

You will have to give in to us or lower your own price. In this way we will cut each other's throats, but you have set the example and are forcing this necessity upon us. And don't think that this is an idle threat. The prospectuses are ready,

¹⁹ STN to Panckoucke, Dec. 18, 1777; Panckoucke to STN, Nov. 19, 1777; STN to Panckoucke, Dec. 7, 1777.

and we have the same type, the necessary presses etc. at our disposition in Yverdon.²⁰

This maneuver forced Duplain to retreat, but it also resulted in open war; for although negotiations continued intermittently—the usual style in eighteenth-century warfare—each side campaigned fiercely, attempting to destroy the other's market.

The octavo group relied on a strategy of smuggling. They filled their subscription and counted on reaching their clients through the underground circuits of the clandestine book trade. The quarto group calculated on blocking those circuits. Panckoucke promised his partners, "I guarantee that they will not penetrate France. The magistrate promised me so. . . . You understand, Messieurs, that being armed with a privilege, you should not concede your rights any more than I. Because of our contracts, our privilege, Duplain had to come make terms with us. The Lausannois will have to do the same."²¹ The system of privilege and protection that had nearly destroyed the first edition of the *Encyclopédie* was being used as the main line of defense in the effort to save its successor. So much had conditions changed from the reign of Louis XV to that of Louis XVI that the government treated *encyclopédisme* more as a commercial than an ideological matter. This new attitude suggests that enlightened ideas permeated the government itself, but it does not necessarily imply weakness; in fact the contest between the strategy of smuggling and the strategy of policing provides a test case of the government's ability to control the printed word.

In mid-1778 the Société Typographique de Neuchâtel sent an agent, Jean-François Favarger, on a tour of southern and central France. Favarger's first assignment was to check the society's supply lines along the French-Swiss border. In Saint Sulpice, the last town on the Swiss side of the border, he learned that the smuggling outfit of Meuron Frères had recently taken care of five 500-pound crates containing volume 1 of the octavo *Encyclopédie*. The Meuron brothers told him so themselves, with more than a hint of professional pride, because they handled the society's own smuggling but only as occasional substitutes for Pion of Pontarlier, the society's first-string smuggler, whom they wanted to replace. On the other side of the border, in Pontarlier, Pion told Favarger that he had seen five *acquits à caution*—a customs permit used by the French state to control imports of foreign books—that had been fraudulently discharged by Capel, syndic of the booksellers' guild in Dijon. Since Capel was officially required to confiscate the books that he forwarded, Dijon now promised to surpass Besançon as the main entrepôt of this underground route, as Favarger announced triumphantly in notifying his employers that the octavo had passed from Bern to Saint Sulpice to

²⁰ Société Typographique de Lausanne to Pellet of Geneva, Nov. 1, 1777, copy included in a letter from the Société Typographique de Lausanne to STN, Nov. 20, 1777.

²¹ Panckoucke to STN, Dec. 22, 1777.

Dijon and was now headed toward Paris. The Société Typographique hid Capel's name in the hope that "for money he will provide us with the same service"²² and relayed the rest of the information to Panckoucke, who alerted the French authorities, who eventually captured the crates. The authorities engineered other confiscations on their own—in Toulouse, for example, where a big bust inflicted huge casualties on the octavo group. By August Favarger's field reports showed that subscribers were deserting the octavo in droves throughout the Midi. And in early 1779 the octavo publishers sued for peace.

The negotiations dragged on for a year, while the quarto group finished a mopping-up operation in France and the octavo group tried to repair its losses through sales in Central and Eastern Europe. Finally in February 1780 Panckoucke sold the entry into France to the Lausanne-Bern consortium for 24,000 livres. That was a steep price—roughly eight per cent of the octavo's current manufacturing cost—and it shows how strong the demand for *Encyclopédies* remained, at least in the calculations of publishers who had discovered a new, undernourished public. Thinking they were safe at last, the octavo group increased their printing to 6,000 copies—hence the explanation of another "missing" edition²³—and promptly fell into another of Panckoucke's traps. Because they had not been able to pay off Panckoucke in cash, they had persuaded him to accept his ransom in kind—that is, in 24,000-livres' worth of octavo *Encyclopédies*. Panckoucke dumped his octavos on the French market at a reduced price and then compounded the damage to the octavo group's future sales by spreading the word that he would soon produce an *Encyclopédie* to end all *Encyclopédies*—not the revised edition that he had originally planned with the Société Typographique de Neuchâtel, but the *Encyclopédie méthodique*, which he was then organizing with the support of a consortium from Liège. That was not the last low blow in this battle, because four years later the old members of the octavo group joined by none other than Panckoucke's former ally, the Société Typographique, announced a plan to pirate the *Encyclopédie méthodique*.²⁴ It did not get far beyond the drawing board, however; so the

²² Favarger to STN, July 8, 1778; STN to Favarger, July 11, 1778.

²³ In the case of this second bibliographical mystery Watts ("Swiss Editions of the *Encyclopédie*," 230–32) and Lough (*Essays on the Encyclopédie*, 40–41) differ slightly, but each did a good job of guessing at the solution, considering that they did not know who was behind the quarto enterprise or how it came into conflict with the octavo edition.

²⁴ By this time the quarto association had been dissolved and the STN had split with Panckoucke and had formed the Confédération Helvétique with the sociétés typographiques of Bern and Lausanne. The plan of the Confédération Helvétique was to reprint all the articles of the *Encyclopédie méthodique* that had not appeared in the earlier editions, to arrange them in alphabetical order—rather than according to subject, as they appeared in the *Encyclopédie méthodique*—and then to sell them, in all three formats, as supplements to the early editions. In this way the owners of the first sets could acquire all the new material of the *Encyclopédie méthodique* by purchasing only a few volumes of supplements, and the Swiss printers could badly damage Panckoucke's potential market.

quarto-octavo war may be said to have ended in the defeat of Lausanne and Bern.

The publishing wars did not cut off the supply of relatively inexpensive *Encyclopédies* to France. On the contrary they show how fiercely publishers struggled to satisfy the French market and how important that market must have been. They also illustrate the aggressive, entrepreneurial character of Enlightenment publishing in contrast to the conservative publishing industry that was dominated by the guild structure within France.²⁵ And finally they expose the inadequacy of the common view that the Enlightenment and the regime were locked into a fight to the death; for the quarto group captured the market by enlisting the state on its side—a strategy of protection and privilege that typifies the ways of the Old Regime and that also suggests a shift in the tone of government in the mid-1770s. The book that had barely survived persecution under Louis XV became a best seller under Louis XVI—with the blessing of the government.

THE LAST EPISODE in the *Encyclopédie* wars was purely domestic, a civil war between Duplain and his associates. In February 1779 they met in Lyons to assess their affairs. Contrary to all expectations, Duplain gave a pessimistic account of the sales. The first two subscriptions had done splendidly, he explained, but that very success had tempted the associates to over-extend themselves, and the third edition now looked like a disaster. They might rescue it, however, if they divided up one thousand unsold sets so that each associate could market them in areas where his sales were normally strongest. Panckoucke accepted this proposal, because the Parisian territory was reserved for him and, anyway, he would allot almost half of his five hundred sets to the Société Typographique. Six months later, in a still gloomier report, Duplain warned that this maneuver had not sufficed to save the third edition. Hundreds of volumes would rot in their warehouses unless they took drastic measures. Fortunately Duplain had found a merchant, a certain Perrin, who had caught the *Encyclopédie* fever, and they could dump their unsold copies on him. To be sure, Perrin demanded extraordinary terms—a fifty-per-cent reduction—but they would be lucky to get rid of their excess stock at any price, and Perrin would take a huge number: 422 sets, as well as 160 from Panckoucke's share of the thousand that had been split between him and Duplain in February. Panckoucke accepted the proposal, but soon after signing the Perrin contract he began to harbor suspicions. He learned that Duplain had tried to involve a mutual friend in a secret conspiracy to raid his reserved quarto market in Paris, and he found that Duplain's letters sounded disturbingly vague about Perrin,

²⁵ For further information on these contrasting types of publishing, see Robert Darnton, "Reading, Writing and Publishing in Eighteenth-Century France: A Case Study in the Sociology of Literature," *Daedalus*, Winter 1971, pp. 214-56.

whom they described as “a commercial agent in Strasbourg, who has a business in Lyons, or rather, I believe, in Paris, anyhow an extremely rich man for whom I can reply.”²⁶ By September 1779 Panckoucke confided to the Société Typographique, “I am quite persuaded that this Perrin is only an imaginary being or, at most, a straw man. Duplain is avaricious and makes no pretense about being delicate.”²⁷ He had become convinced that Duplain was “a vile soul,” “a voracious man, who loves money with a fury”; “his rapacity has no limits.”²⁸ And he advised the Société Typographique to slip a spy into Duplain’s shop. They needed no prompting, for they had done so long ago. In fact all the associates spied on each other. Panckoucke had his own man watching Duplain; the Neuchâtelois received secret reports on Panckoucke; they kept an agent in Geneva; and their man in Lyons spun such a web of industrial espionage that they finally trapped Duplain in February 1780.

The Lyonnais network managed to track down the elusive Perrin, who indeed turned out to be a straw man in Duplain’s pay, and then it made an even bigger catch: it got hold of a copy of a secret subscription list, Duplain’s record of the actual number of *Encyclopédie* sales. The list made no reference to the Perrin sale; instead it contained 978 more subscriptions than Duplain was later to report at the final settling of accounts in February 1780. The Société Typographique suspected the fraud before this meeting and verified it, once Duplain made his report, by writing to the booksellers whose subscriptions had been falsified, according to a comparison of the reported subscriptions and the secret list. So it discovered that the flow of orders never had dried up, as Duplain had claimed. On the contrary, the entire third edition had been sold at the normal price, except for the five hundred sets that Duplain had dumped on Panckoucke. Duplain had hidden the sales in order to collect the full amount from them, while paying nothing for five hundred of the *Encyclopédies* that he sold and paying for the rest at half price through the phony intermediary of Perrin.

Instead of contenting himself with this spectacular double swindle, a matter of more than 200,000 livres, Duplain piled fraud on fraud in combinations too complex to be fully explained here. His role as general administrator of the enterprise offered enormous opportunity for speculation, because the quarto association allotted him set amounts for all his expenses. He therefore contracted the printing to the lowest bidder, pocketing the difference between what he was allotted and what he paid. He also cheated on the costs of paper and transport and even collaborated in a technique of fraudulent spacing and paragraphing worked out by a Genevan printer—an item that might have seemed trivial to a lesser embezzler but that ex-

²⁶ Panckoucke to STN, Sept. 10, 1779, citing a letter he had received from Duplain.

²⁷ Panckoucke to STN, Sept. 27, 1779.

²⁸ Panckoucke to STN, Nov. 6, 1778, Mar. 7, 18, 1779.

panded volume 19 by 96 unnecessary pages, worth 744 livres. Panckoucke and the Société Typographique calculated that Duplain's kickbacks and rake-offs came to 127,000 livres, but that was only an estimate, one that probably did not do justice to his genius. His intentionally unintelligible accounts could have concealed far more speculation, because they scrambled more than three million livres of expenses and revenues, and Duplain seems to have cheated at every possible point. For example, he attributed 494 subscriptions to the Lyons firm of Audambron and Jossinet at the usual reduced price for booksellers of 294 livres plus one free set for every twelve subscribed, which brought their total up to 535 subscriptions. The anti-Duplain network discovered that Audambron and Jossinet operated as a false front to hide the fact that Duplain had sold all 535 sets at the full subscription price of 384 livres, thereby robbing the association of 60,204 livres.

Since the quarto enterprise had been conducted like a conspiracy from the beginning, it exploded in the end like the denouement of a *drame bourgeois*—or an “English cockfight,” as the Société Typographique put it.²⁹ The anti-Duplain forces had concealed their suspicions while they accumulated enough ammunition to destroy Duplain at the final meeting for the settling of accounts at Lyons in February 1780. This strategy of counterdissimulation had not been easy, as the Société Typographique confessed to Panckoucke: “You have wisely counseled us to dissimulate with him until the very end and not to reveal our just discontent, but by devil it gets more and more difficult every day.”³⁰ When the showdown came, therefore, Duplain's associates surprised him with a barrage of accusations that they had been preparing for almost a year. They produced a correct version of the accounts, exposing a spectacular string of embezzlements. They unveiled the Perrin affair; they stripped the camouflage from Audambron and Jossinet; and they produced the secret subscription list with letters from booksellers testifying to the enormity of the swindles in sales. Even then Duplain refused to break down and confess. So they raided his office with a police commissioner, an attorney, and a bailiff, demanding confiscation of his papers; and they turned his family and friends against him, threatening to ruin the family's name by revealing the entire affair to the public. Finally Duplain surrendered. He agreed to compensate his partners with 200,000 livres, if they would sweep everything under the rug, where it has remained until today.

What sort of a man was this Duplain? The question has a certain fascination, both for economic history and for the history of the human soul. Duplain was a robber baron of the book trade, a gambler who played off high risks against high profits and who made a business of Enlightenment. He decided to stake everything on the quarto *Encyclopédie*. He sold his shop,

²⁹ F.-S. Ostervald and Abram Bosset DeLuze, codirectors of the STN, writing to the STN from Lyons, Jan. 29, 1780.

³⁰ STN to Panckoucke, Mar. 14, 1779.

his stock of books, his house, and his furniture and moved into a furnished room in order to concentrate exclusively on the great affair. Then he hit the jack pot; for this supreme gamble made him a rich man, even after the settlement of 200,000 livres. And once he knew he was wealthy, Duplain began to buy. First he acquired a wife, a beautiful young Lyonnaise who dazzled Panckoucke; then an estate in the provinces; finally the office of *maître d'hôtel du Roi*—that is, nobility. He began signing his letters “de St. Albine.” He served the king for the requisite time in Versailles and lived with his bride in offensive luxury in Paris before carrying her off to his château.

What is the moral of this story? It is a Balzacian drama: the tale of a bourgeois entrepreneur who clawed his way to the top and then consumed his fortune conspicuously, in aristocratic abandon. It is a saga of fortunes made and *illusions perdues* in publishing. In a way it is the story of French capitalism. And its supreme irony is that the vehicle for Duplain’s rise into France’s archaic hierarchy, only a few years away from destruction, was Diderot’s *Encyclopédie*. Perhaps Duplain’s story may also serve as a warning against placing too much confidence in sociological analysis of the sort that follows; for even if you can put a man perfectly in some socioeconomic category, his heart may be elsewhere. Duplain, the perfect bourgeois capitalist, turns out to be a pseudonoble—or was pseudonobility the essence of the French bourgeoisie?

THE INSIDE STORY of the warfare among the men who produced the *Encyclopédie* may reveal something of the spirit of entrepreneurial capitalism in early modern France, but it does not answer the larger question of what the battles were all about. Of course “booty capitalism” was waged for booty. Panckoucke and the pirates, Duplain and the Swiss, and their supporting cast of financiers, smugglers, and traveling salesmen all realized that they could make a fortune by satisfying the vast market in France for a “popular” edition of the supreme work of the Enlightenment. The ferocity of the competition to supply that demand suggests that the interest in enlightened ideas had spread very widely throughout France—to a *grand public* if not a mass audience. But what was the character of that public? That question, like so many problems in the sociology of literature, is difficult to resolve, but one can measure the outside boundaries of the readership of the *Encyclopédie*. First it is necessary to review the basic facts about all the editions of Diderot’s text; then it should be possible to calculate the economic limits to their different consumption patterns; and finally one can attempt to chart the geographical and social distribution of the quarto editions, which were by far the most numerous in prerevolutionary France.

Aside from the Italian editions published (in French) in Lucca and Leghorn, the expurgated Protestant *Encyclopédie* published in Yverdon by Barthélemy de Félice, and the *Encyclopédie méthodique*—a completely

reorganized work that ran to 202 volumes and was not completed until 1832—Diderot's text went through four main metamorphoses.³¹

(1) The first edition (1751–52): this was a folio edition consisting of 17 volumes of text and 11 plates, followed by a five-volume *Supplément* and a two-volume *Table Analytique*. There were 4,225 sets printed, but only half, or perhaps merely a quarter, of them were sold in France. The subscription price was 980 livres, and the market price in the 1770s varied from 1,200 to 1,500 livres.

(2) The Genevan reprint (1771–76): it had the same number of folio volumes in a printing of 2,200 sets. The subscription price was 794 livres, but by June 1777 it was selling at 700 livres, owing to competition from the quarto editions.

(3) The three quarto “editions” (1771–81): these correspond to Duplain's three subscriptions and appeared under the names of Pellet and the Société Typographique de Neuchâtel, as explained above. The quartos contained 36 volumes of text and three volumes of plates. They included 8,011 sets in all and were almost entirely sold out at the subscription price of 384 livres—the price paid by individual subscribers; booksellers subscribed at a reduced price of 294 livres and received one free copy for every dozen they ordered.

(4) The two octavo “editions” (1778–82): these were really one expanded edition representing two subscriptions, published at Lausanne and Bern. The octavos consisted of 36 volumes of text and three of plates. They included 6,000 sets in all, and each sold at a subscription price of 231 livres.

This enumeration of facts and figures suggests a surprising conclusion: there were far more *Encyclopédies* in prerevolutionary France than anyone—except eighteenth-century publishers—has ever suspected. Although the subscription figures in the publishers' papers make it difficult to calculate precisely how many copies remained in the kingdom, they permit a safe estimate: between 14,000 and 16,000 *Encyclopédies* existed in France before 1789, and half of them can be traced. So without pretending to know how many of those *Encyclopédies* were read, or in what way the readers responded to them, it seems legitimate to hypothesize that *encyclopédisme*

³¹ The following information comes from the sources cited above (nn. 1, 2), except for the figures on market prices and on the sizes of the quarto and octavo printings, which come from the papers of the STN. The extraordinary richness of those papers makes it possible for the first time to estimate accurately the total volume and cost of the *Encyclopédie* trade in pre-revolutionary France. The octavo publishers originally announced that their edition, which followed the quarto page by page, would sell at a subscription price of 195 livres—6 livres down, 5 livres for each volume of text, and 15 livres for each volume of plates. When they learned that the quarto would run to 36 volumes of text instead of 29, as was originally planned, they had to follow suit and therefore charged 231 livres for their subscriptions—contrary to what has been affirmed by Watts (“Swiss Editions of the *Encyclopédie*,” 231) and by Lough (*Essays on the Encyclopédie*, 40). On the subscription prices of the octavo, see also the *Gazette de Berne*, Nov. 19, 1777, and Apr. 8, 1780.

could have spread far more widely through French society than is generally believed.

As the *Encyclopédie* progressed from edition to edition its format decreased in size, it contained fewer plates, its paper declined in quality, and its price went down. And as the publishing consortia succeeded one another, they cast their nets more and more widely, reaching out with each new edition toward remoter sections of the reading public. The price differential set some rough limits to this ever-broadening sales pattern: the quarto edition cost a little more than one-fourth and the octavo edition about one-fifth of the market price of the first folio in the 1770s. But what were the social boundaries of *Encyclopédie* "consumption"? The question may seem impertinent, since economics offers no explanation of what it is to "consume" a book and since book buying and book reading are quite different activities. Nonetheless, the purchase of a book is a significant act when considered culturally as well as economically. It provides some indication of the diffusion of ideas beyond the intellectual milieu within which cultural history is usually circumscribed. And as there has never been a study of the sales of any eighteenth-century book, a sales analysis of the most important work of the Enlightenment ought to be worthwhile.

One can estimate how closely the *Encyclopédie* came into contact with the lower classes by translating its price into bread, the key commodity of the Old Regime and the basic element in the diet of most Frenchmen.³² A first folio *Encyclopédie* was worth about 3,500 loaves of bread and a quarto 960 loaves, the standard of measurement being the "normal" price of 8 sous for a four-pound loaf of rye bread in prerevolutionary Paris. An unskilled laborer with a wife and three children would have to buy at least 18 loaves a week to keep his family alive. In good times he would spend half his income on bread. A "cheap" quarto *Encyclopédie* therefore represented more than a year of his family's precarious nutriment. It would have been as inconceivable for him to buy it—even if he could read it—as for him to purchase a palace. Skilled laborers—locksmiths, carpenters, and printers—made 15 livres in a good week. The first folio would have cost them 93 weeks' wages, the quarto 26 weeks' wages, and the octavo 15½ weeks' wages. So even the upper strata of the working classes, artisans like the men who printed the book, could never have afforded to buy it.

But the men who wrote it, the "Gens de Lettres" invoked on its title page, could have purchased the cheaper editions. Diderot himself made an average of 2,600 livres a year for his thirty years of labor on the *Encyclo-*

³² The following information on artisans' "budgets" and the price of bread comes from the work of Ernest Labrousse, George Rudé, and Albert Soboul. For a convenient summary of their findings, see Rudé, *The Crowd in the French Revolution* (Oxford, 1959). The "Banques des ouvriers" in the STN papers, contain full information on the wages of the printers of the Société Typographique. Oddly enough, they corresponded exactly to the wages of skilled workmen in Paris, where the cost of living was higher.

pédie.³³ A quarto would have cost him 7½ weeks of his wages and an octavo 4½—not an extravagant sum, considering that he had other sources of income. Many writers were wealthier than Diderot, thanks to patrons and pensions. B. J. Saurin, a typical figure from the upper ranks of the Republic of Letters, now deservedly forgotten, made 8,600 livres a year in pensions and “gratifications.”³⁴ He could have treated himself to a quarto, the equivalent of 2⅓ weeks’ income. The octavo was for hack writers like Durey de Morsan, a literary adventurer who lived off the crumbs from Voltaire’s table and who wrote as one of the octavo’s “zealous subscribers” to the Société Typographique de Neuchâtel:

The number of poor literary men far surpasses that of rich readers. I myself am delighted that this work, too expensive until now, does not exceed the means of the semi-indigent such as myself. I would like the door of the sciences, of the arts, and of useful truths to be open, day and night, to every human who can read.³⁵

It is impossible to produce typical figures for the wide variety of incomes among the middling classes of the provinces, but the following calculations should give some idea of the expensiveness of the *Encyclopédie* for persons located well below the great noblemen and financiers and well above the common people. Although curés received only 500 livres as their *portion congrue* after 1768, their annual income often amounted to 1,000–2,000 livres.³⁶ So a quarto *Encyclopédie* represented ten weeks’ income for a prosperous curé. Magistrates of the baillage courts stood at the top of the legal profession among provincial bourgeois and often earned 2,000–3,000 livres a year: a quarto *Encyclopédie* was worth six or seven weeks of their income. To live “noblement” a bourgeois had to count on at least 3,000–4,000 livres a year in *rentes*: the purchase of a quarto *Encyclopédie* would have taken five weeks of his revenue.³⁷

In strictly economic terms, therefore, the first two editions were so expensive that they cannot have penetrated far beyond the restricted circle of courtiers, salon lions, and progressive *parlementaires* who made up the cultural avant-garde. The cheaper editions were luxury items, but with some squeezing they could have been made to fit into many middle-class budgets, rather as encyclopedias do today. The cost, like the content, of the quarto and octavo *Encyclopédies* appealed to a wide variety of small-town notables

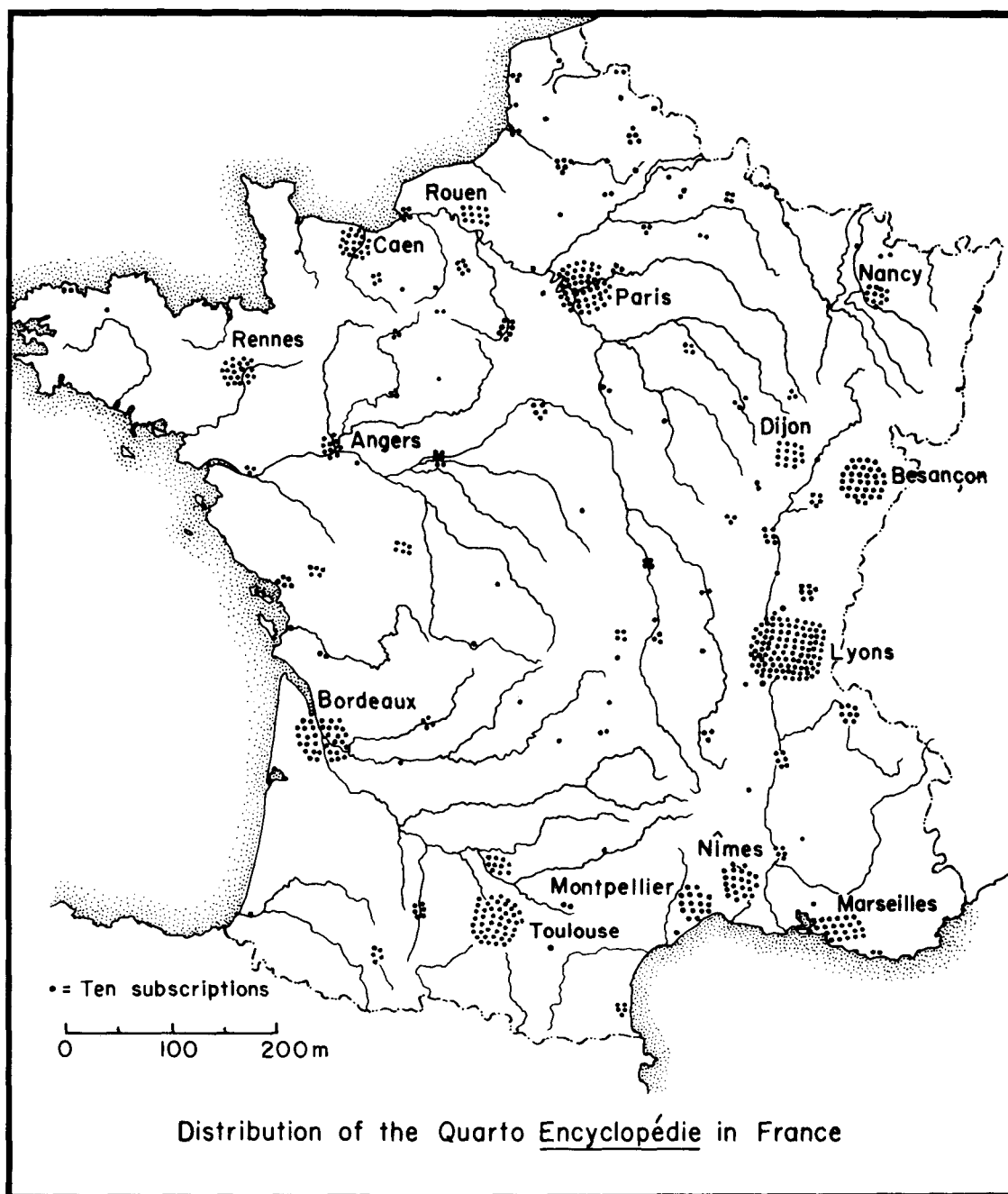
³³ Proust, *Diderot et l'Encyclopédie*, 59; for a detailed study of Diderot’s income, see pages 81–116.

³⁴ Robert Darnton, “The High Enlightenment and the Low Life of Literature in Prerevolutionary France,” *Past and Present*, no. 51 (1971): 87.

³⁵ Durey de Morsan to F.-S. Ostervald of the STN, Apr. 17, 1778.

³⁶ Marcel Marion, *Dictionnaire des institutions de la France aux XVII^e et XVIII^e siècles* (Paris, 1923), 446; Henri Sée, *La France économique et sociale au XVIII^e siècle* (Paris, 1933), 64–66. The *portion congrue* allotted to every curé from the revenue of the *dîme* (tithe) was increased in 1786 to 700 livres.

³⁷ Philip Dawson, *Provincial Magistrates and Revolutionary Politics in France, 1789–1795* (Cambridge, Mass., 1972), ch. 3; Sée, *La France économique et sociale*, 162.



This map is drawn up from Duplain's secret subscription list, MS 1220 in the Bibliothèque de la Ville de Neuchâtel. The list covers all but one of the 8,011 sets printed. Of these 828 were foreign and so do not figure on the map. The map also excludes 76 sets that were sold to unidentified individuals and 25 sets that were given away—either as compensation for employees and associates or to procure protections; at least 10 of these went to Lyons, and Panckoucke dispensed 4 in Paris. The unidentified sales all involved single sets, except in four cases, which appear on the list simply as "Ollier 6," "Vasselier 4," "La Flèche 39," and "Berage 8." "La Flèche" could have been a person but probably represents La Flèche, Maine, where there was a famous school, originally founded for the Jesuits by Henry IV. The large number of copies sold in Lyons as compared with Paris resulted in part from the way the business was handled: Duplain directed the marketing operations from Lyons, while Panckoucke's many affairs kept him too busy to be much of a salesman in Paris. Also, the Parisian market was probably pretty well supplied by earlier editions. This map therefore should not be taken to prove that the capital of the Enlightenment absorbed relatively few *Encyclopédies*. What it provides is a fairly accurate picture of *Encyclopédie* diffusion in the provinces.

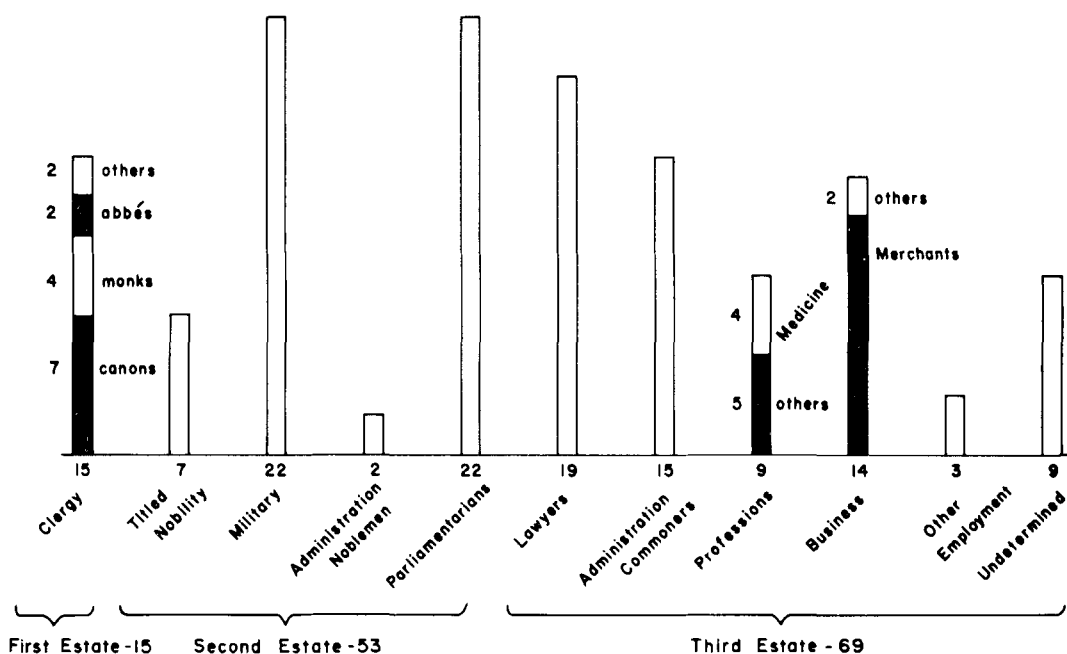
SUBSCRIPTIONS TO THE QUARTO *ENCYCLOPÉDIE* IN FRANCE

Abbéville	26	Castelnaudary	27	Macon	17	Rethel	40
Aire	8	Castres	28	Mantes	8	Roquemaure	7
Aix	6	Chalon-sur-Saône	67	Marseilles	228	Rouen	125
Alençon	34	Châlons-sur-		Meaux	30	St. Chamond	2
Amiens	59	Marne	1	Metz	22	St. Didier	1
Angers	109	Champagne	2	Montargis	26	St. Etienne	13
Argentin	3	Chartres	75	Montauban	105	St. Flour	24
Arras	26	Chatillon	39	Millau	8	St. Lô	7
Auch	65	Clermont	13	Montbrisson	6	St. Omer	5
Aurillac	13	Colmar	1	Montpellier	169	St. Quentin	16
Autun	39	Dijon	152	Morlaix	1	Saintes	26
Auxerre	10	Dôle	52	Mortagne	22	Saumur	1
Auxonne	1	Embrun	3	Moulins	52	Sedan	2
Avignon	55	Evreux	65	Nancy	120	Sète	13
Bayonne	16	Falaise	45	Nantes	38	Soissons	52
Beaune	26	Douai	14	Nîmes	212	Strasbourg	2
Beauvais	8	Grenoble	80	Niort	58	Tarbes	52
Bergerac	13	Gueret	19	Noyon	26	Thiers	39
Bergues	1	La Fère	15	Orléans	52	Toulon	21
Besançon	338	Langres	26	Paris	487	Toulouse	451
Billom	2	Laon	17	Perigueux	36	Tours	65
Bordeaux	356	La Rochelle	56	Peronne	15	Troyes	53
Boulogne-sur-Mer	34	Le Havre	52	Perpignan	52	Tulle	4
Bourg	91	Le Mans	40	Poitiers	65	Valence	65
Bourg-Saint-		Le Puy	39	Reims	24	Valenciennes	13
Andéol	4	Lille	28	Rennes	218	Verdun	12
Bourges	20	Limoges	3	Riom	46	Versailles	4
Brest	20	Lisieux	27	Roanne	26	Vichy	2
Caen	221	Lunéville	1	Rochefort	27	Villefranche	37
Cambray	57	Laigle	3				
Carpentras	2	Lyons	1078				

and country gentlemen but not to anyone below the bourgeoisie. As the publishers remarked—and they knew their clientele—“The in-folio format will be for grands seigneurs and libraries, while the in-quarto will be within the reach of men of letters and interested readers [*amateurs*] whose fortune is less considerable.”³⁸ The *Encyclopédie* entrepreneurs realized that they could widen their profit margin as they broadened their market. They had discovered a gold mine of untapped literary demand, and their scramble to exploit it shows how advanced culture reached the general reading public. But where were those readers located, and who were they?

The map (see p. 1348), drawn up from Duplain’s secret subscription list, shows the geographical distribution of almost all the quarto copies, that is approximately half the *Encyclopédies* that existed in prerevolutionary France. It demonstrates that the *Encyclopédie* reached every corner of the country and that its distribution coincided fairly well, as far as one can tell, with the distribution of population. Subscriptions in the Parisian area and the northwest were few, perhaps because those markets were sated by other editions. Beyond Rennes, Brittany looks like an intellectual desert, which might have been the case, but a surprising fertile crescent of *Encyclopédies*

³⁸ STN to Rudiger of Moscow, May 31, 1777.

Subscribers to the Quarto *Encyclopédie* in Besançon

This bar graph is drawn from the list of individual purchasers of the quarto edition in Lough, *Essays on the Encyclopédie*, 466-73. It contains the names and *qualités* of 253 subscribers from the Franche Comté, of whom 137 were from Besançon. Duplain's secret list shows there were 390 subscriptions sold in the province, a figure that is confirmed by letters from the two booksellers who collected them. Therefore the representativeness of the Comtois list, which was drawn up according to the order in which the subscriptions arrived, is far from being complete—it amounts to two-thirds of the subscriptions sold. But the last third of the subscribers probably tended to come from outlying areas of the large, mountainous province, and so the bar graph probably gives a fairly accurate picture of the subscription pattern within Besançon. The military category seems to have been made up entirely of noblemen—most had titles but are not entered under "titled nobility"—but the "parlementaires" probably included an undetermined number of commoners, so the second estate appears somewhat larger on the graph than it was in reality. The same may be true of the third estate, because some of the "undetermined" category could have been noblemen. The three men represented by "other employment" were identified on the list as "intendant du Prince de Bauffremont," "Conseil de Mgr. le Duc du Châtelet," and "garde-magasin," presumably an army position.

curves through the Midi, from Lyons to Nîmes, Montpellier, Toulouse, and Bordeaux. Even the Massif Central shows a fairly high density of subscriptions. So there is little evidence here for the hypothesis that France was divided into a backward south and a progressive north by the "Maggiolo line" of literacy, running from Mont St. Michel to Geneva.³⁹ The *Encyclopédies* seem to have sold best in towns where there were parlements and academies, but it sold very well everywhere: that is probably the main conclusion to be drawn from the map. Once reincarnated in a comparatively cheap edition, Diderot's text traveled farther and wider than has been appreciated.

³⁹ Michel Fleury and Pierre Valmary, "Les Progrès de l'instruction élémentaire de Louis XIV à Napoléon III," *Population*, no. 1 (1957): 71-92. Of course there is no reason to expect that the diffusion of the *Encyclopédie* should coincide with the primitive level of literacy indicated by Fleury and Valmary.

Duplain's secret subscription list does not identify all of the subscribers; it contains only the names of booksellers, who generally bought lots of a dozen or more sets, which they retailed among their local clients. But there is one list of individual purchasers of the quarto edition in the Franche Comté. It has been translated into the bar graph (see p. 1350), which covers Besançon, a judicial, administrative, ecclesiastical, and military center, where sales were unusually strong. The graph shows a high percentage of purchasers in the legal profession, both lawyers and members of the parlement of Besançon. The *Encyclopédie* sold well in the first two estates, and especially among noblemen in the army, as might be expected in a garrison town. Royal administrators, almost all of them nonnoble, also bought the book in large number, and so did bourgeois professional men, particularly doctors, though to a lesser extent. Fourteen of the 137 sets went to merchants and manufacturers—a large proportion in comparison with Daniel Roche's statistics on provincial academicians and Jacques Proust's analysis of the contributors to the *Encyclopédie*.⁴⁰ Approximately one-half of one per cent of the people in Besançon bought the quarto *Encyclopédie*—a high percentage, but one that seems credible, given the above economic analysis of cost and clientele. The town's two main booksellers, Lépagnez and Charmet, had not expected to sell more than a dozen or so sets and were astounded at the book's success, especially as their trade had fallen into a slump since 1777. "Please don't believe that I enjoy any great consumption of books here," Lépagnez wrote to the Société Typographique. "I swear to you that after *L'Histoire universelle*, *L'Histoire ecclésiastique*, that of the Gallican Church, the Bible of Vance, the *Encyclopédie*, and the Rousseau, everything else has given me no business at all for the last two years."⁴¹

The sales pattern of Besançon may not have been typical of France as a whole, but nonstatistical information shows a similar enthusiasm for the *Encyclopédie* in other provincial centers. In Toulouse, at the other extremity of the kingdom, a bookbinder called Gaston sold 182 quartos in three weeks and expected to place 400 octavos. And in general, when French booksellers mentioned their quarto clients in their correspondence they named lawyers, royal officials, and local noblemen—unlike their counterparts in Northern, Central, and Eastern Europe, who referred only to courtiers. So all the evidence points in the same direction: in prerevolutionary France the *Encyclopédie* worked its way into the world of the provincial notables who assumed the leadership of the Revolution and who continued to dominate the countryside throughout the nineteenth century.

No one can pretend to know what message "took" in the minds of those readers. Many of them must have bought the *Encyclopédie* for what it

⁴⁰ Daniel Roche, "Milieux académiques provinciaux et société des lumières," in François Furet et al., *Livre et société dans la France du XVIII^e siècle* (Paris, 1965), 93–185; Proust, *Diderot et l'Encyclopédie*, ch. 1.

⁴¹ Lépagnez to STN, Aug. 30, 1780.

claimed to be: a compendium of all knowledge, rather than philosophic propaganda. As Panckoucke put it, "The *Encyclopédie* will always be the first book of any library or cabinet"⁴²—but it could have been a book to display on shelves, not to read. In fact Panckoucke reported that some subscribers in Lyons could not read at all. But it is difficult to believe that a high proportion of its owners never got through even its Preliminary Discourse, which is a manifesto of the Enlightenment. And far more people must have read the *Encyclopédie* than owned it, as would be common in an era when books were liberally loaned and when *cabinets littéraires* were booming. It therefore seems legitimate to conclude that the biography of this book—the protection accorded it by French authorities, the struggle to exploit it among bookdealers, and its diffusion among a clientele of middle-range notables everywhere in the country—that this extraordinary success story reveals an Enlightenment that had spread far beyond the elite of court and capital and had penetrated throughout the upper echelons of the Old Regime. As the Société Typographique wrote to a customer in August 1779:

Never has an enterprise of this kind and this scope had a greater success, nor has one been conducted with such speed. In less than two and a half years, and after having renewed the subscription twice, we have printed 8,000 copies of this *Encyclopédie*, of which we have only a small number yet to sell. The public seems to have waited impatiently to be served by publishers less rapacious than the producers of the first edition [a dubious statement]. We and our associates pride ourselves in having satisfied it in this respect; and you will observe, Sir, that if Enlightenment [*lumières philosophiques*] lacks in this best of all possible worlds, it will not be our fault.⁴³

⁴² Panckoucke to STN, Aug. 4, 1776.

⁴³ STN to J. G. Bruere of Homburg, Aug. 19, 1779.

The Historical Problem of Generations

ALAN B. SPITZER

EACH GENERATION writes its own history of generations. Or perhaps, when contemporary generational differences force themselves on the consciousness of historians they rediscover significant age-specific relationships in the past. Given our recent past, the current preoccupation with past generations was predictable, but as is appropriate for historians we shall probably run somewhat behind events, flooding the market with histories of generations just when our present generational crisis has evaporated.¹ There is nothing wrong with this—our responsibility does not lie in being up-to-date but in the effective application of what has been a vague, ambiguous, and stretchable concept to the explanation of past events.

¹ A sampling of recent works in the history of (predominantly European) generations includes: Anthony Esler, *Bombs, Beards and Barricades. 150 Years of Youth in Revolt* (New York, 1971); Esler has also mined this theme in *The Aspiring Mind of the Elizabethan Younger Generation* (Durham, 1966) and in his chapter, "Youth in Revolt: The French Generation of 1830," in Robert Bezucha, ed., *Modern European Social History* (Lexington, 1972), 301–34; Lewis S. Feuer, *The Conflict of Generations: The Character and Significance of Student Movements* (New York, 1969); Daniel R. Browder, "Fathers, Sons and Grandfathers: Social Origins of Radical Intellectuals in Nineteenth-Century Russia," *Journal of Social History*, 2 (1969): 333–55; Herbert Moller, "Youth as a Force in the Modern World," *Comparative Studies in Society and History*, 10 (1968): 237–60; Phyllis H. Stock, "Students versus the University in Pre-World War Paris," *French Historical Studies*, 7 (1971): 93–110; Phillipe Bénéton, "La Génération de 1912–1914. Image, mythe et réalité?" *Revue française de science politique*, 21 (1971): 981–1009; Peter Lowenberg, "The Psychohistorical Origins of the Nazi Youth Cohort," *AHR* 76 (1971): 1457–1502; Herbert Butterfield, *The Discontinuities Between the Generations in History* (Cambridge, 1972); William J. McGrath, "Student Radicalism in Vienna," *Journal of Contemporary History*, 2, no. 3 (1967): 183–201; the entire issue of *ibid.*, 5, no. 1 (1970) is devoted to "The Conflict of Generations"; Michael A. Ledeen, "Fascism and the Generation Gap," *European Studies Review*, 1 (1971): 275–83. Somewhat earlier efforts include: Marvin Rintala, *Three Generations: The Extreme Right Wing in Finnish Politics* (Bloomington, 1962); Rintala, "The Problem of Generations in Finnish Communism," *American Slavic and East European Review*, 17 (1958): 190–202; Rolland Ray Lutz, Jr., "Fathers and Sons in the Vienna Revolution of 1848," *Journal of Central European Affairs*, 12 (1962): 161–73; John Eros, "The Positivist Generation of French Republicanism," *Sociological Review*, new series 3 (1955): 255–77; Yves Renouard, "La Notion de Génération en Histoire," *Revue Historique*, 209 (1953): 1–23; Sigmund Neumann, "The Conflict of Generations in Contemporary Europe: From Versailles to Munich," *Vital Speeches of the Day*, 5 (1939): 623–28. In my opinion the best historical treatment of a particular generation is still Louis Mazoyer, "Catégories d'âge et groupes sociaux. Les Jeunes Générations françaises de 1830," *Annales*, 10 (1938): 385–423.

The parallel development by sociologists, political scientists, and demographers of a literature devoted to a systematic analysis of contemporary generations provides a methodological resource for historians, although the social scientists also fall into the tendency, characteristic of generational studies, of a slippery, ambiguous usage that blurs distinctions which should be clarified.² It will be my contention that clarity can be preserved and useful explanations developed if instead of asking how long a generation really is, or how many generations usually coexist, or what points in the individual's life cycle are decisive, or whether aging has more profound political consequences than early socialization, we ask whether, and in what respects, age-related differences mattered in a given historical situation.

Modern empirical studies of generations proceed from the theoretical contributions of Karl Mannheim. Most historians accept Mannheim's classic formulation: "The social phenomenon of 'generations' represents nothing more than a particular kind of identity of location, embracing related 'age-groups' embedded in a historical-social process." They follow Mannheim and other pioneers such as François Mentré and José Ortega y Gasset, in distinguishing between generations identified through familial succession—the biological chain from father to son to grandson—and generations conceived as groups of coevals, people of roughly the same age whose shared experience significantly distinguishes them from contemporaries in other age groups. Some demographers prefer to reserve the term "generation" for the familial succession and apply "cohort" to the group of coevals, but historians have generally retained the traditional term with a qualifier that indicates a significant shared experience, writing of "social" or "political" or "literary" generations.³

² Some of the standard approaches from the perspective of the social sciences are: Karl Mannheim, "The Problem of Generations," in his *Essays on the Sociology of Knowledge* (London, 1959), 276–322; S. N. Eisenstadt, *From Generation to Generation: Age Groups and Social Structure*, (Glencoe, 1956); Rudolf Heberle, *Social Movements: An Introduction to Political Sociology* (New York, 1951); Bennett M. Berger, "How Long Is a Generation?" *British Journal of Sociology*, 11 (1960): 10–23; Marvin Rintala, "A Generation in Politics: A Definition," *Review of Politics*, 25 (1963): 509–22; see also Julián Marías, "Generations: The Concept," and Martin Rintala, "Generations: Political Generations," in the *International Encyclopedia of the Social Sciences* (1968), 6: 88–96. There is a recent immense accumulation of literature on youth as such, distinguished by contributions from Erik Erikson, Kenneth Keniston, Richard Flacks, Seymour Lipset, and others. See the bibliographical article by John Somerville, "Toward a History of Childhood and Youth," *Journal of Interdisciplinary History*, 3 (1972): 439–47.

³ Mannheim, *Essays on the Sociology of Knowledge*, 292; François Mentré, *Les Générations sociales* (Paris, 1920); José Ortega y Gasset, *The Modern Theme* (New York, 1961); Ortega, *Man and Crisis* (New York, 1958); Norman R. Ryder, "The Cohort as a Concept in the Study of Social Change," *American Sociological Review*, 30 (1965): 843–61; the recent translation of Julián Marías's *Generations. A Historical Method* (University, Alabama, 1970) provides the best survey of the theory of generations in Comte, Mill, Wilhelm Dilthey, Ortega, and so forth. It also covers the classic works on literary generations. On the latter, see also, Julius Petersen, "Die Literarischen Generationen," in Emil Ermatinger, ed., *Philosophie der Literaturwissenschaft* (Berlin,

Such adjectives provide a certain focus for the application of the concept but do not resolve fundamental problems of definition, or the essential problem of establishing the boundaries of any presumed generation. That problem is defined by the most telling argument against any historical explanation based on generations—to wit, “There’s one born every second.” This observation seemingly disposes of all the theories that divide centuries into three generations, or substitute generational spans for traditional periodization, or discover a dialectical alternation of types of generations. Johan Huizinga provides a classic statement of the objection:

A triad of generations 1700–33, 1734–69, and 1770–1800 is proposed, by means of which a number of historical phenomena, together constituting the history of the eighteenth century, are considered in the sequence rise, maturity and decline—or action, reaction and synthesis. But there can just as easily be a series of generations marked by the years 1701–1734, 1735–70, and 1771–1801, and so on for every year, and actually for every day. . . . The theory is more valid when applied to one specific and well-defined cultural phenomenon. But even then its validity is deceptive, for the generation in itself, considered biologically, is always quite arbitrary, and can never be held responsible for an evolutionary phase of a specific historical phenomenon.⁴

Another formidable statement of dissent was contributed by Lucien Febvre, who showed what little comfort was left in confining the theory to “one specific and well-defined cultural phenomenon”; for this very limitation is an admission that any general chronological definition cuts across significant particular age groups. There is no *a priori* guarantee that a literary generation, for example, will be chronologically congruent with a political generation which can be identified in roughly, but not precisely, the same time span. Even if such generations happen to be perfectly congruent chronologically they may not share the attributes that set them apart from their predecessors and successors, for “there is no guarantee that the *political* generations of 1660 and 1690 are set apart by the differences and for the reasons that divide the literary generations of 1660 and 1690.”⁵ Furthermore, the historical treatment of generations invariably refers to only a segment of the age group under consideration. The attributes of a “youth” composed of French intellectuals will have little relation to their Chinese coevals, or more to the point, to French peasants and workers of the same age.

These objections have been recognized by theorists of generations, who usually apply some variant of Mannheim’s “generation unit” to

1930), 130–87; Detlev W. Schumann, “Cultural Age-Groups in German Thought,” *PMLA*, 51 (1936): 1180–207; and Henri Peyre, *Les Générations littéraires* (Paris, 1948).

⁴ Johan Huizinga, *Men and Ideas* (New York, 1965), 73–74.

⁵ Lucien Febvre, “Générations,” in *Bulletin du centre internationale de synthèse. Section de synthèse historique*, no. 7, p. 41, published in *Revue de synthèse historique*, 47 (1929).

the social group or cultural phenomenon they wish to isolate with reference to birth dates. According to Mannheim, "Youth experiencing the same concrete historical problems may be said to be part of the same actual generation; while those groups within the same actual generation which work up the material of their common experiences in different specific ways, constitute separate generation units."⁶ We must all make do with something like Mannheim's distinctions whenever we wish to generalize about age-specific behavior without asserting the identity of all those within the relevant cohort. Yet it can be argued, as Febvre did argue, that the identification of political, intellectual, religious, economic, social, and some number of other variables that might plausibly differentiate one generation unit from another requires distinctions so fine and complex as to reduce the ambitious concept to a "useless" and "parasitical" notion.⁷

THE MOST SYSTEMATIC attempt to meet these and other serious objections to any historical theory of generations is in the recently translated book of Julián Marías, *Generations. A Historical Method*. It contains a thorough survey of the literature on the subject, and an attempt, through a consolidation and exposition of Ortega y Gasset's fragmentary writings on generations, at a definitive resolution of the issues raised in the literature. Marías ranges over the entire history of the concept of generations, but the core of his argument is concentrated in the pages where he shows how generations can be reconstructed "empirically" through the application of Ortega's principles.⁸ Ortega and Marías begin with the definition of a generation as a group born within a zone of dates and sharing "a structure of *vigencias*"—the binding customs, collective usages, traditions, and beliefs that define the real social existence of each individual. The dimensions of each zone of dates approximately correspond to the fifteen-year span that Ortega assigns to each overlapping but historically distinct age group. These age groups are characterized by the fairly familiar categories of childhood, youth, initiation, dominance, and old age. The age of dominance, for example, subsumes those aged forty-five to sixty, who usually run the world and who share a not completely separate but appreciably different structure of *vigencias* from the preceding and succeeding generations.

⁶ Mannheim, *Essays on the Sociology of Knowledge*, 304. There are two gradations in this concept: only those coevals who shared significant experience would comprise a generation, and only those who worked out their problems in the same way would be members of the same generation unit.

⁷ Febvre, "Généérations," 42.

⁸ Marías, "Ortega's Theory of Generations," in *Generations*, 69–106.

Mariás is aware that this approach is vulnerable to Huizinga's objection. What justifies the arbitrary selection of one zone of dates instead of another? Why refer to an age of dominance for those aged forty-five to sixty in 1965, rather than those forty-five to sixty in 1964 or 1966? The answer is revealed in the concept of "the decisive generation," the one that "for the first time thinks the new thoughts with full clarity and with complete possession of their meaning, a generation that is neither still a precursor nor any longer bound by the past." The decisive generation is identified or reconstructed through the discovery of the individual who "most clearly represents the essential characteristics of a period," when the "full bloom of a new era" occurs.⁹ Thus Descartes is identified as the "eponym" of a decisive generation, and the date of his thirtieth birthday becomes a tentative point of departure from which other generations can be fixed by adding or subtracting multiples of fifteen. The generational center of the decisive generation might actually fall on the twenty-eighth or the thirty-fourth rather than on the thirtieth birthday of Descartes, but empirical investigation will reveal the appropriate birthday.¹⁰

Mariás and Ortega recognize major objections to this approach but dispose of them with reference to "the empirical content of the human past." Their brand of empiricism consists of magisterial assertions about significant individuals in the history of ideas who are characterized as anomalous, or representative, or eponymous with regard to their epoch. If one does not choose to widen one's historical lens to Ortega's focus on two hundred years of "historical crisis" resolved by a spiritual renaissance commencing with Galileo and culminating in Descartes one is not likely to be persuaded of the eponymous individual in the decisive generation. But even if one believes that a new era blossomed between 1600 and 1650 and that its essential characteristics were represented by Descartes, one need not agree that whatever was particularly significant in Descartes was substantially shared by his coevals, or that the essential contributions of Descartes had much to do with generational phenomena which cannot be subsumed under intellectual history, or that the fundamental transformation separating the generation of Descartes from its predecessors would subsequently be significantly modified at something like

⁹ Mariás, *Generations*, 100. Ortega y Gasset, *Man and Crisis*, 62. Cf. *ibid.*, p. 61: "Take a great historic ambit within which a change in human living has been brought about which is fundamental, visible and unquestionable."

¹⁰ Mariás (*Generations*, 172-76) presents a tentative variation on the Ortegian approach for periods in which it is difficult to locate the decisive generation or the representative figure. He locates more or less representative figures born fifteen years apart, clusters the names of other important coevals around each, and then adds chronological layers to each core, year by year, until an age group seems anomalous in one of the original categories but appropriate to its predecessor or successor, at which point he has established the boundary between two generations. It is difficult to see the need for this, for if Mariás believes in the decisive representativeness of Descartes, and the permanent validity of the fifteen-year intervals, he can simply add on fifteen-year layers from the seventeenth to the nineteenth century.

fifteen-year intervals. Some of this might be rendered plausible by research or even demonstrated; in Ortega and Marías it is merely asserted.¹¹

Furthermore there is a circularity in the Ortegian approach characteristic of those generational theories which define a phenomenon in a way that provides an explanation of its historical effects. It is perfectly acceptable to identify *vigencias*, or generation units, as clusters of attributes that distinguish groups chronologically, but it is something else to explain the behavior of those groups with reference to a chronological definition constructed out of the evidence of that very behavior. That is, if a historically significant cohort is defined as all those whose experience of the First World War decisively affected their political behavior in 1939, questions about the generational consequences of World War I are answered by definition.

I do not present these criticisms as a counsel of despair because I believe that a certain methodological modesty can disarm the standard objections put with such clarity by Huizinga and Febvre. The problem posed by Huizinga: how to specify the boundaries of generations in the seamless continuum of daily births? is a problem for anyone who chooses to mark off categories in any continuum. In this sense specifying generations is no more arbitrary than specifying social classes, or ideologies, or political movements where there is inevitably a shading off or ambiguity at the boundaries of categories. Indeed the most chaste behavioralism often creates arbitrary categories—as tall, medium, or short; extremely anti-imperialist, moderate, extremely proimperialist; and so forth. I would even argue that such categories as 5'9" or 160 lbs. suffer from the same defects as "the generation born between 1792 and 1802." Demographers, after all, feel no qualms in manipulating categories presented to them by the arbitrary decisions of the Bureau of the Census, inserting in their pyramids the cohort of "males aged 25–30 in 1960," without wondering whether they might not have used "left-handers aged 27–31 in 1958." Where we suspect that age-specific differences are historically significant we can quite appropriately cut age groups out of the continuum to see whether observations of their documented collective behavior and their relation to other groups can contribute to plausible explanations. Of course it may be that their behavior is not sufficiently distinct to set them off from older or younger groups in any useful way.

The same considerations apply to the class of objections raised by Lucien Febvre. His criticism is actually directed against two different ways in which units of generations are used to identify the entire age group. The first has to do with generation units suggested by categories of collective behavior that presumably can be distinguished along

¹¹ For a historian who accepts the Ortegian method with certain qualifications see, Renouard, "La Notion de Génération en Histoire."

generational lines, as in literary or political generations. The second refers to minorities of age groups which are presumed to characterize the entire generation, such as a "youth" consisting of a radical minority of college students.

The first method of identifying generations presents a problem only because of slovenly usage or of familiar habits of expression. When one identifies a literary generation that persists for some fifty years, one is really saying that despite the differences in the socialization and life experience of individuals who were not coevals there were no significant age-specific differences with regard to literature during the period. A fifty-year generation makes people uneasy so they try to chop it up into decent fifteen- or thirty-year intervals.

While slovenly, the chronological stretching of the term "generation" is often perfectly intelligible. When Alexander Portnoy says that he belongs to the generation of network radio and eight teams to a league he is locating himself in a population born perhaps between 1890 and 1935, but he has identified a cultural category in which age differences do matter without foreclosing other ways of slicing up the population. On the other hand the chronological stretching of the term sometimes obscures significant age-specific experiences and blurs useful historical distinctions. One might identify a Positivist generation in France extending from 1850 to 1900, but this is little help in understanding the persistent differences among Positivists, Cousinian spiritualists, and devout Catholics that characterized the intellectual life of the period. However the attack on Positivism after 1900 was manifested along generational lines, at least in contemporary polemic.¹²

Febvre's correct remark that such a unit as a literary generation may not be substantially identical with a coeval political generation need not inhibit us if we can document significant age-specific differences in the particular subject or field of collective behavior under consideration. But this does not dispose of the objection to the presumption that some minority incarnates or represents an entire generation. The question often has a polemical edge—for example, when the characterization of a "youth" depends on one's response to the assumption that a radical intelligentsia expresses the general will of its coevals.

This issue is endemic in many areas of political and social analysis. Identifying radical college students with youth in general raises the same questions as characterizing the entire black population by urban militants, or all Protestant churches by Prohibitionists. The point is that we can reject false claims to identity or even representativeness

¹² Eros (in "The Positivist Generation of French Republicanism") does identify a specific generation of the young Republican politicians of the 1870s whose Positivist formation distinguished them from their predecessors. They constituted the aging establishment attacked by the anti-Positivists of the turn of the century.

without denying that significant distinctions may depend on the relevant minority. We might discover both that Prohibitionists were only a minority of Protestants and that Prohibitionism separated the Protestants from other sects. George Rudé's investigations of the social composition of the revolutionary crowd cannot demonstrate that his militant workshop masters, craftsmen, wage earners, shopkeepers, and petty traders actually represented their social groups, but he can show that insofar as such groups played a significant political role they did it through that militant minority.¹³

WE ARE AFTER THE way in which the unit contributes to an explanation of collective differences. We want to avoid the imposition of categories in ways that blur or obscure significant differences. Blurring and obscuring are chronic to the generational approach because distinct age-specific phenomena are often jumbled into the same historical generation. This problem is recognized in the recent research of political scientists and sociologists who will, for example, distinguish between life cycle and generation behavior—the first referring to recurrent behavior appropriate to the chronological phases of every individual's life span, and the second, as emphasized by Mannheim, Marias, Rudolf Heberle, and others, reflecting the distinct collective experiences of given age groups, which stamp those age groups with a permanent separate identity as they move through time. The first approach is as old as the conception of the Ages of Man; the second is often applied by historians with reference to a social trauma or a "Great Divide," as in the identification of a World War or Depression generation. Neither of these categories need be identical with what occurs when, as Mannheim puts it, "individual members of a generation become conscious of their common situation and make this consciousness the basis of their group solidarity."¹⁴ The articulation of this shared consciousness is more or less what is meant by a generational ideology.¹⁵ If the division (however perceived) between generations is greater than that normally attributed to life-stage differences we have what is currently called a generation gap.

¹³ George Rudé, *The Crowd in the French Revolution* (Oxford, 1959). A different way of looking at this issue is by identifying and controlling relevant variables. For instance, in evaluating the significance of age for collective behavior, social scientists often control for education; see, for example, Samuel A. Stouffer, *Social Research to Test Ideas* (Glencoe, 1962), 121–24. It is also possible to examine generational identities as phenomena of more profound social divisions; see, for example, Georg Lukács, "Balzac: Lost Illusions," in *Studies in European Realism*, (New York, 1964), 47–64.

¹⁴ Mannheim, *Essays on the Sociology of Knowledge*, 290.

¹⁵ Eisenstadt (*From Generation to Generation*, 102, 311) defines a "youth ideology" that affirms "youth culture" as a distinct type of social and cultural life. Collective affirmation of a generational ideology has usually been embodied in a youth movement, but could in principle just as well be articulated by older cohorts.

These historically overlapping but conceptually distinct generational categories by no means exhaust the ways in which collective behavior might correlate with age. When, for example, we have identified age-specific differences in political attitudes that constitute a significant generation gap we have not necessarily demonstrated that these differences "will endure and transform culture."¹⁶ We are more like our fathers than we like to think, and dramatic generational conflicts have often been softened or eroded by time and the stamp of culture until the rebellious youths assume in maturity the commitments and lifestyles of their predecessors. This is sometimes the case even when a generation has suffered a historical trauma presumed to mark it for life—the cataclysms of the First World War, the Second World War, the German occupation, and the Liberation did not liberate Frenchmen from the characteristic political institutions of the Third and Fourth Republics.

On the other hand, there are fundamental changes, manifested first as a generational break, that become permanent and are transmitted through successive age groups until they characterize the entire population. Many observers believe that the differences between older and younger cohorts of French farmers represent such a turning point, or more broadly, that the most fundamental change in French life since 1789 lies in the patterns of social and economic behavior that have distinguished those born after 1930 from their predecessors.¹⁷

Just as specific investigation is required to differentiate the presumed historical consequences of being young, it is called on to verify assertions regarding the effects of aging on collective behavior. American political scientists have been especially concerned with the relationship between aging and political attitudes.¹⁸ The same easy generalizations and hidden complexities obtain for received opinions associating aging with increased conservatism as for those identifying youth and rebellion. To cite a familiar example: When one has identified a correlation between old age and conservative attitudes one still has to establish whether that cohort has become increasingly conservative with age or has retained attitudes,

¹⁶ This is an insight of the sociologist Philip Abrams. For the concept of the life cycle Abrams coins the term *age span*, the "culturally defined phases of the individual life cycle which may be empirically observed in any society"; for the phenomenon of generational solidarity he introduces *age groups* shaped by the "collective consciousness crystallized within an age span . . . creating meaningful (linking or disassociating) relationships between it and other age spans"; and he assigns the term *generations* to age groups that "not only repudiate norms established by their seniors but carry that repudiation with them through life and seek to transmit it through their successors." Philip Abrams, "Rites de Passage," *Journal of Contemporary History*, 5, no. 1 (1970): 175-90.

¹⁷ See, for example, John Ardagh, *The New French Revolution* (New York, 1969), 67-68.

¹⁸ For an early proposal to explore this relationship, see, John Schmidhauser, "The Political Behavior of Older Persons: A Discussion of Some Frontiers of Research," *Western Political Quarterly*, 11 (1958): 113-24.

considered relatively liberal or radical in its youth, that have come to rest at the Right of a shifting political spectrum.

An age cohort may, then, be differentiated from the rest of the population because its attitudes persist while those of the majority change. In such a case differences that begin as political or ideological may end as generational. A political elite that wrests power from its coeval enemies may hold it long enough to become a gerontocracy increasingly distinguished from the mass of the population by attitudes preserved from its heroic receding past. Perhaps this is the phenomenon to which Chou En-lai referred when he commented on the relative youth of President Nixon's entourage.

There are many ways in which age differences of no particular significance are transformed and sharpened by changing objective realities. Where we find a developing correlation between old age and resistance to tax-supported education, we may be observing an appropriate collective response to the deteriorating financial situation of older people rather than some constitutional crabbedness inevitably associated with aging. The introduction of military conscription creates an immediate, vital age- (and sex-) linked distinction that virtually imposes a generational self-identity on those of draft age.

The actual historical situation of any age group is defined in practice by its relationship to other cohorts, even with regard to size. Norman Ryder's observation that "a cohort's size relative to the sizes of its neighbors is a persistent and compelling feature of its lifetime environment"¹⁹ has been tragically verified in this century by the effects of the virtual obliteration of entire generations. As Sigmund Neumann suggested over thirty years ago, the demographic consequences of the First World War are not exhausted by the skewed pyramids of the demographers. On the eve of the Second World War he emphasized the "over-age" of the political leaders of France and Britain who "had to maintain positions which should have been filled by millions of young men lost in the World War and cheated of their share in making a new world."²⁰ This suggestive insight into the complex, remote consequences of an erosion that separated generations by something like a demographic trench was rather blurred by Neumann because he fused it into a discussion of the crucial conflict between the prewar political cohorts and the surviving members of the wartime generation. He was concerned to advance what has become one of

¹⁹ Ryder, "The Cohort as a Concept in the Study of Social Change," 845. From a somewhat different angle Bennett Berger emphasized the difference in conceptions of relative age associated with different occupations—a baseball player is "old" at thirty-five, a presidential candidate "young" at fifty. Berger, "How Long Is a Generation," 15.

²⁰ Neumann, "The Conflict of Generations in Contemporary Europe," 627; he covers roughly the same ground in *The Permanent Revolution* (New York, 1965), 230–56.

the familiar interpretations of the Nazis—as a political generation separated from their elders not because they were decimated by the war but because they were socialized in the trenches.

Neumann's approach reflects the classic distinction between contemporaries and coevals argued by generational theorists such as Mannheim and Ortega who emphasize that generations at different phases of the life cycle experience the same events in different ways.²¹ Young soldiers fight and die while older cohorts mourn and rule. However, one cannot deduce the historical significance of a particular generational relationship from the fact that "every moment in time . . . is always experienced by social generations at various stages of development."²² It remains to be established by research and analysis.

Indeed any assumption of a relationship between age groups and behavior of interest to historians needs to be established by investigation of each particular case. Marvin Rintala's assertion that "no shared destiny is more fundamental than that of the same generation" cannot be refuted (or verified) if it is a statement about human essence, but it is of no help in understanding specific historical context. There have been many situations in which class, racial, sexual, religious, or linguistic differences were far more significant than those related to age. The questions to be put to the data are suggested by Philip Abrams's remarks on so-called political generations: "We must ask in what circumstances differentiation springing from the social organization of age will crop into age-linked political conflict, and finally we must ask in what particular circumstances such conflicts will be defined in terms of a conflict of generations rather than anything else."²³

In answering such questions the familiar generalizations about the recurrent characteristics of phases of the life cycle are not always helpful. To explain the alienation of young Frenchmen in the early 1830s and young Americans in the late 1960s by the chronic tropism of the young for radicalism, idealism, frustrated mobility, oedipal hostility, and so forth, contributes little to explanations of youthful passivity and careerist pragmatism in the 1850s or the 1950s. Again Mannheim cleared the conceptual ground with the observation that factors presumed to be present in every situation cannot explain "the particular features of a given process of modification."²⁴ Of course

²¹ This concept is persuasively applied in Claude Digeon's *La Crise Allemande de la pensée française (1870-1914)* (Paris, 1959), where characteristic responses to the catastrophe of 1870-71 are identified for generations of 1830, 1850, 1870, and 1890.

²² Mannheim, *Essays in the Sociology of Knowledge*, 283.

²³ Rintala, "A Generation in Politics," 509; Abrams, "Rites de Passage," 181.

²⁴ Mannheim, *Essays on the Sociology of Knowledge*, 312. In *ibid.*, 297 n., Mannheim characterizes "the fundamental thesis of this essay . . . that biological factors (such as youth and age) do not of themselves involve a definite intellectual or practical orientation (youth cannot be automatically correlated with a progressive attitude and so on); they merely *initiate* certain formal tendencies, the actual manifestations of which will ultimately depend on the prevailing

one might construct an explanation out of the clichés about youthful alienation by assuming that the periods of rebellion were the norm, other things being equal, and that the task is to identify the intervening variables that precipitated those abnormal eras when youth was apolitical and acquiescent—as if parents were to ask, “What is the matter with that well-behaved boy?”

There are times, as at the present, when significant generational differences seem confined to the conflicts between youth and everybody else. This encourages the tendency to consider those historical developments that are linked to age groups solely in relation to the generation gap.²⁵ Significant generational differences are then reduced to the conflict between father and son, the biological succession of generations is confused with the historical succession of age cohorts, and assumptions regarding patterns of behavior common to youth at any time and place are fused with descriptions of specific experiences that stamp a permanent collective identity on a given generation.²⁶

A RECENT WIDELY DISCUSSED attempt to specify the historical circumstances in which the life-stage of youth attains a particular, and malignant, coherence is Lewis S. Feuer's *The Conflict of Generations*. Professor Feuer ranges widely across time and space to identify and explain the implications of dynamic student movements from the German *Burschenschaften*, through a century and a half of generational rebellions in Europe, Africa, and Asia, to the Berkeley student uprising of the late sixties. Feuer's concentration on student movements is consistent with his title, because he argues that a politically dynamic student movement always reflects a conflict of generations. His concern is not merely to demonstrate where and how youth movements have mattered, but to identify the recurrent ele-

social and cultural context. Any attempt to establish a direct identity or correlation between biological and cultural data leads to a *quid pro quo* which can only confuse the issue.”

²⁵ See, for example, Anthony Esler's *Bombs, Beards and Barricades*, which is subtitled *150 Years of Youth in Revolt*. Esler does distinguish the nonrevolting youth of other times and places from those in the Western world who have been revolting ever since the impact of the Democratic and Industrial Revolutions, *ibid.*, 34. This is virtually the conclusion of Konrad Lorenz, who finds modern youth extraordinarily revolting in “The Enmity between Generations and its Probable Causes,” *Psychoanalytic Review*, 57 (1970): 334-404. Lorenz believes that the process of family disintegration that began with the Industrial Revolution has deprived youth of the indispensable transmission of tradition, except in “certain lucky old-fashioned peasant families.” Herbert Moller (in “Youth as a Force in The Modern World”) makes a demographic distinction between periods when youth is a relatively small proportion of the population and things are reasonably quiet, and periods when a large proportion of the population is young and things go to hell in a hand basket.

²⁶ For the observation that the concept of youth as a distinct life stage between adolescence and adulthood is not universal, that it is both historically contingent and confined to a minority of the age group, see Kenneth Keniston, “Youth: A New Stage of Life,” *American Scholar* 39 (1970): 631-54; see also the influential if controversial views of Philippe Ariès, *Centuries of Childhood*, tr. R. Baldeck (New York, 1962).

ments that give them a characteristic stamp and predictable consequences. These elements might be summed up by the terms "idealism" and "irrationality," attributes explicable, according to Feuer, by the psychological matrix of all such generational conflict. Without repeating the criticisms of Feuer's tendentiousness in selecting, ordering, and interpreting evidence to confirm his antipathies,²⁷ I wish to comment on those flaws in his conceptual apparatus that suggest recurrent problems in historical explanations of generational conflict.

The major conceptual flaw in *The Conflict of Generations* is in the causal model that provides the explanatory force and interest of the work. In principle at least, Feuer's method allows him to discriminate, and to explain the differences, between rebellious and conformist youthful generations. Universal characteristics of youth contribute to the characteristic form of youth movements but do not guarantee the rise of those massive and militant student movements that only appear with the "de-authorization" of the older generation "as a collective whole."²⁸ In the particular occasions of the de-authorization of the fathers an explanation is found not only for the radical alienation of the sons but for the recurrent political expressions of this alienation. The recognition of the oedipal springs and the parricidal guilt of the generational rebellion helps us to understand the self-sacrificing idealism, the populism, and the murderous and suicidal irrationalism of militant youth movements.

As Feuer travels across his immense blighted generational landscape he traces a somewhat circular path because he begins with a definition of a student movement as a combination of students moved by "disillusionment with and rejection of the values of the older generation."²⁹ He would probably answer that he has in fact identified a locus of collective parental loss of authority for each case of destructive youthful rebellion—fixing historically the psychological antecedents of behavior that could never be understood in strictly ideological or sociological terms. But Feuer cannot establish that a militant, irrational youth had reason to reject its fathers' authority unless he can demonstrate that the generation's irrational militants actually experienced the psychological process of parental de-authorization. Feuer does this with selected individuals such as Mao Tse-tung or Karl Follen, the leader of the German student movement after the Napoleonic Wars. However, these individual examples—which themselves do not

²⁷ For example, Richard Flacks's review article in *Journal of Social History*, 4 (1970-71): 141-53; Marshall Meyer's review in *American Journal of Sociology*, 75 (1969): 293-95; for a review which admits the flaws but is fundamentally sympathetic, see Henry A. Murray, *The American Scholar*, 38 (1969): 710-16.

²⁸ Feuer, *The Conflict of Generations*, 184.

²⁹ A point made in a review by Arthur Liebman, in *American Sociological Review*, 34 (1969): 1012.

bear careful scrutiny—cannot validate an explanation of collective behavior.³⁰

Feuer's variant of the "Oedipal-rebellion" hypothesis is rejected by all those who hold what Kenneth Keniston calls the "red-diaper-baby" theory, based on evidence that today's young rebels are characteristically the children of yesterday's radicals. Of course it is perfectly possible that contemporary studies which find "continuity with parental values to be the rule and discontinuity the exception"³¹ cannot be generalized to other times and other places, but in any case generalizations about the individual antecedents of collective behavior cannot be verified solely with reference to the collective behavior. That is, one cannot explain collective behavior with reference to specific antecedent experiences when one cannot provide evidence for the antecedent experiences.

Attempts to do this often fall into the ecological fallacy—the assumption that the relationships of properties of groups are identical with the relationships of properties of individuals within the groups. This is particularly tempting when one wishes to emphasize the generational consequences of early socialization, where it is assumed that because a large percentage of a given age group has had a certain experience at time 1, and a large percentage of the same age group engaged in a certain form of behavior at time 2, the two percentages represent the effects of the experience in time 1 on the behavior in time 2.

A stimulating example of this fallacy is Peter Lowenberg's article, "The Psychohistorical Origins of the Nazi Youth Cohort," which asserts a correlation between the political behavior of young Nazi voters in 1932 and their childhood experience of nutritional deprivation, absence of parents, and failure of public authority during the First World War. Applying Freudian conceptions of fixation and regression, Lowenberg argues that the traumatic wartime experiences of those born roughly between 1900 and 1910 resulted in a "weakened character structure manifested in aggression, defenses of projection and displacement, and inner rage," which revealed itself politically as a result of the renewed trauma of the Great Depression, in the preference

³⁰ Feuer's treatment of Follen is certainly unpersuasive. To support the characterization (in *The Conflict of Generations*, 59) of Follen's prototypical conflicts with his father, Feuer quotes a passage about Follen's resentment at having been teased by his father, without giving an inkling of the following passage from the same text: "It was during this period, that the strict and tender union commenced between Charles and his father, which combined all the holiness of a natural affection with all the peculiar pleasures of a tender friendship. . . . This tender, this unlimited indulgence established a peculiar feeling of intimacy and of confiding love between him and his father, such as few boys are blessed with." What the entire passage seems to establish is that Follen's relations with his father were deeply affectionate but not without friction. Charles Follen, *The Works of Charles Follen* (Boston, 1841), 1: 5-9.

³¹ Kenneth Keniston, *Youth and Dissent* (New York, 1971), 273-74.

of the cohort for "extremist paramilitary and youth organizations and political parties."³²

Lowenberg likes the generational approach because it "deals with probabilities—with the law of averages on a macroscale—thus canceling out any of the many individual variables that determine conduct."³³ But the law of averages on a macroscale does not eliminate the *post hoc* element in his model or guarantee that those individuals who actually experienced the relevant early trauma disproportionately voted Nazi in 1930 or 1932, or that the young Nazi voters had not enjoyed a significantly more secure, stable, and well-fed childhood than that of the young voters whose relative mental health led them to join the paramilitary organizations of the Social Democrats.

It is conceivable that the younger cohorts of northern Protestant agricultural, lower-middle-class, and self-employed voters who swung to nazism, or the young urban workers who voted Communist, suffered greater childhood deprivation than the young inhabitants of Berlin and Hamburg working-class districts who stuck with the SPD, but Lowenberg presents no evidence to that effect. Nor does he present evidence that would impel one to prefer his model to standard explanations of the propensity of young people in general, and students in particular, to turn to extreme solutions when traditional alternatives have failed. He does more or less feed this interpretation into his explanation, along with the thesis that assimilates the political responses of young Germans, especially students, to other groups vulnerable to economic dislocation, chronic underemployment, and the threat of proletarianization. He also grants some force to the thesis emphasizing continuities between the prewar youth movement and Nazi appeals to a postwar youth. Presumably he believes that these relationships are not sufficient conditions for such pathological political behavior as voting Nazi or Communist in 1932, without the additional variable of the early trauma.³⁴ My argument is not that this view is impossible, but that Lowenberg fails to demonstrate that traumatized youth voted in a manner significantly different from nontraumatized youth, or to explain why the "second trauma" of the depression and the other factors that impelled older cohorts to vote for the Nazis do not sufficiently account for the Nazi sympathies of youth.

A somewhat different psychohistorical origin is identified by Anthony Esler in his study of the rebellious French youth of 1830. They characteristically experienced a pattern of parental overindulgence fol-

³² Lowenberg, "The Psychohistorical Origins of The Nazi Youth Cohort," 1501.

³³ *Ibid.*, 1464.

³⁴ I am not concerned here with Lowenberg's debatable assumptions that to vote Nazi or Communist in 1932 was in some sense pathological or neurotic behavior and that such behavior was psychodynamically congruent with the early trauma.

lowed by extreme repression at school, and "this pattern of permissiveness in childhood yielding abruptly to repressiveness in adolescence surely helped to create the smoldering sense of injustice that burst out at last in the generational rebellion of the 1830's."³⁵ Without dwelling on the psychodynamic assumptions buried in Professor Esler's "surely," one might still point out that his model does not help to distinguish this rebellious generation from any other cast in the same familial mold; nor does it establish a significantly different background for the nonrevolutionary generation units of 1830.

Something rather like the ecological fallacy is argued in Rolland Lutz's attempt to relate social class to the political role of the generation of the Viennese "sons" who were at the radical core of the revolution of 1848. Noting the lower-class origin of a considerable minority of students at the University of Vienna he remarks, "What could be more natural than the assumption of leadership over the Vienna masses by the educated sons of provincial shopkeepers and artisans?"³⁶ This assumption is fortified by no evidence that the poor students were the most militant. There seems no reason to prefer Lutz's assumption to the contemporary American discovery that militant students are more likely to be drawn from upper-class educated families than from the ranks of the ambitious poor.

In arguing these points it is not my intention to smother fruitful hypotheses under a blanket of methodological Pyrrhonism. This article proceeds from the assumption that age-specific relationships are sometimes of great historical significance. The problem is to decide which kind of age relationship is specifically relevant and, therefore, to identify explanatory model and criteria of verification that are appropriate to the specific relationships under investigation.

BECAUSE ATTEMPTS TO verify generalizations about the effects of aging, early socialization, or other generational phenomena have not come to much, some social scientists have concluded that there is little to be gained from a generational approach.³⁷ Yet others have assembled evidence indicating that age differences do matter for certain groups under certain circumstances. Such evidence is usually drawn from survey research, which can be useful, despite various limitations, to historians of the recent past. My purpose, however, is not to remind those historians of something they already know or to suggest that historians imprison themselves in the methodological preferences of sociologists and political scientists. The point is that surveys of generational phe-

³⁵ Esler, in Bezucha, ed., *Modern European Social History*, 308.

³⁶ Lutz, "Fathers and Sons in *The Vienna Revolution*," 167.

³⁷ See, for example, H. Hyman, *Political Socialization* (Glencoe, 1959), 139-54.

nomena, particularly when periodically readministered to age cohorts, suggest and clarify the various and distinct ways in which age categories relate to collective behavior. It might be instructive, therefore, to contemplate some examples of cohort analysis in the social sciences as well as certain methodological issues raised in the literature.³⁸

The most familiar and least informative ordering of data related to age is a cross-sectional survey with the age group as the independent variable and an attitude expressed at the time of the survey as the dependent variable, as, for example, in table 1. V. O. Key cites this table to suggest some relation between fundamental shifts in opinion and subsequent generational differences, but he grants that one can draw few firm conclusions from it: "The numbers of the sample do not suffice to permit analysis to tie these differences definitely to age, but if they are so connected, the more conservative views of those of the earlier generation may reflect a strong attachment among them to the values of an earlier era."³⁹ But even if the sample were adequate, the only discernible connection would be that differences of opinion at the moment of the survey over the proper scope of government welfare were to some extent related to age.

We cannot say whether the more restrictive views of older white-collar workers reflect a stronger attachment to an earlier era, a natural consequence of aging, the permanent effect of early socialization, an unusual temporary generational difference, or the attitudes of the same age cohorts in other occupations. Furthermore the age categories

TABLE 1. AGE IN RELATION TO OPINIONS ON PROPER
SCOPE OF GOVERNMENT WELFARE ACTIVITY AMONG
WHITE-COLLAR RESPONDENTS

<i>Opinion</i>	<i>Per Cent under 35</i>	<i>Per Cent 35-55</i>	<i>Per Cent over 55</i>
Should do more	28	28	23
Doing about right	51	43	34
Should do less	15	25	39
Don't know	6	4	4
	100	100	100
N	148	226	106

Source: V.O. Key, *Public Opinion and American Democracy* (New York: Alfred A. Knopf, Inc., 1967), 255.

³⁸ There is still some point in Mannheim's observation: "The present status of the problem of generations thus affords a striking illustration of the anarchy in the political and cultural sciences where everyone starts out afresh from his own point of view." *Essays in the Sociology of Knowledge*, 287.

³⁹ V. O. Key, *Public Opinion and American Democracy* (New York, 1967), 255.

may be too gross for the distinctions we wish to make. Someone interested in the attitudes of postadolescents would find little comfort in such a category as "under 35." However, more finely discriminated cohorts would come no closer to answering the questions posed above if such cohorts were taken from a single cross-sectional survey. What is required is evidence arranged longitudinally—age-related data gathered through time. The most familiar ordering of such data is in arbitrarily defined age cohorts re-examined in successive surveys, which are often separated by uniform intervals.

The systematic manipulation of cohort data is nothing new, at least in the field of demography, but the other social sciences have only recently begun to explore its possibilities. William Evan's article on the cohort analysis of survey data, published in 1959 and often cited as a pioneering effort, was the cutting edge of what has become a fairly substantial literature.⁴⁰ Evan's intention was to introduce and illustrate "the cohort technique, which for present purposes will be roughly equated to a generational analysis, as a means of inquiring into the impact of types of historical events on the opinions, attitudes or ideologies of different generations." For his example Evan traced cohort opinions on government control of railroads, through polls successively taken in 1937, 1945, and 1953 (see table 2).

Perhaps the most useful message delivered by Evan's table is that questions about political or other effects of generational differences must be answered empirically with regard to specific situations. As we follow Evan's cohorts A and B through sixteen years we note what might be considered an increasing conservatism about government control, but not a tendency for older groups to become progressively more conservative than their younger contemporaries. The comparison of the opinions of cohort A in 1937 and cohort X in 1953 indicates that the relative interventionism of the younger cohort in the earlier survey was not a permanent attribute of youth as such. Indeed the juxtaposition of the attitudes of all the cohorts with those of the total population suggests that the increased hostility to government control was general and not age-specific in any significant sense.

The various age-related distinctions expressed in small percentage differences offer little food for reflection except perhaps in the tendency of middle-aged cohort B to shift opinion less sharply than did its younger contemporaries in the course of the sixteen years. Evan notes

⁴⁰ William M. Evan, "Cohort Analysis of Survey Data: A Procedure for Studying Long-term Opinion Change," *Public Opinion Quarterly*, 23 (1959): 63-72. For a more recent examination of the methodological problems of applying cohort data to generational analysis see Neal E. Cutler, *The Alternative Effects of Generations and Aging upon Political Behavior: A Cohort Analysis of American Attitudes toward Foreign Policy, 1946-1966*. (Oak Ridge, 1968), especially chapter 4.

TABLE 2. OPINIONS OF TWO COHORTS ON GOVERNMENT OWNERSHIP OF RAILROADS, IN 1937, 1945, AND 1953 (IN PER CENT)

<i>Opinion and Year of Cross-sectional Survey</i>	<i>Total Population</i>	<i>Cohort A 24-30 in 1937</i>	<i>Cohort B 47-53 in 1937</i>			
1937 ^a						
Yes	25	27		23		
No	58	54		60		
No opinion	17	19	—	17	—	—
	<u>100</u>	<u>100</u>		<u>100</u>		
	(2855)	(618)		(344)		
		<i>Cohort A 32-38 in 1945</i>	<i>Cohort B 55-61 in 1945</i>			
1945 ^b						
Yes	20	21		25		
No	63	64		59		
No opinion	17	15	—	16	—	—
	<u>100</u>	<u>100</u>		<u>100</u>		
	(1584)	(252)		(155)		
		<i>Cohort X 24-30 in 1953</i>	<i>Cohort A 40-46 in 1953</i>	<i>Cohort Y 47-53 in 1953</i>	<i>Cohort B 63-69 in 1953</i>	
1953 ^c						
Should	14	15	12	16	19	
Should not	74	73	78	74	69	
No opinion	12	12	10	10	12	
	<u>100</u>	<u>100</u>	<u>100</u>	<u>100</u>	<u>100</u>	
	(1527)	(247)	(233)	(257)	(108)	

^a "Do you believe the government should buy, own, and operate the railroads?"

^b "Do you think the government should own the railroads in this country?"

^c "Do you think the United States government should or should not own the following things in this country? How about the railroads?"

Source: William M. Evan, "Cohort Analysis of Survey Data: A Procedure for Studying Long-Term Opinion Change," *Public Opinion Quarterly*, 23 (1959):67.

that differences between age groups were smaller in a given year than were differences between identical age groups in different years; that is, the difference between cohort A and cohort B in 1937 or 1953 is smaller than the difference between cohort A in 1937 and cohort X in 1953, or cohort B in 1937 and cohort Y in 1953. This impels him to

conclude "that the historical situation has a greater impact than aging in opinion change."⁴¹ He might just as well, or better, have said that with regard to government control of railroads age differences didn't matter much.

Despite its rather unexciting substantive results, Evan's essay is a useful, and influential, introduction to the systematic analysis of age cohort phenomena and has been succeeded by more detailed and statistically complex attempts to establish correlations between age and collective behavior.

A STATISTICALLY SIMPLE attempt which suggests, more or less unintentionally, the pitfalls in the manipulation of cohort data is Seymour Lipset and Everett Ladd's examination of the politics of college-educated generations. Their observations are based on the findings summarized in table 3. According to the authors, the most obvious generational phenomenon revealed by this table is "a persistent age association in the voting preferences of the college 'generations,'" so that "the younger the voter the greater the preference for the more liberal nominee," and vice versa. It is this phenomenon that suggests the tentative conclusion: "In so far as we can generalize, Aristotle's emphasis on the moderating effects of growing older turns out to be more predictive than Mannheim's theory of the long-term consequences of the early political experiences of 'generation-units.'"⁴² The fact that older generations are more conservative than their successors does not, however, establish the moderating influence of growing older. We cannot know whether the older cohorts became more conservative as they aged unless we have established a base with which to compare their subsequent development. If we rearrange some of the data in a manner that helps us to think diagonally (see table 4), we will see that the information provided by Lipset and Ladd only partially supports their interpretation. To take an example, if we follow the cohort that attended college in 1934-38 we note that, starting with its solid Republican bias in 1948 it fluctuated from Right to Left with the rest of the electorate in 1956 and 1964, to come to rest in 1968 roughly in the position it had held twenty years before.

The authors do recognize that differences related to age may not so much reflect an absolute change in the attitudes of particular generations as a change in the position of the generation relative to the entire population. Since "the historical slope of political attitudes

⁴¹ Evan, "Cohort Analysis of Survey Data," 69.

⁴² Seymour Martin Lipset and Everett Carl Ladd, Jr., "College Generations—from the 1930's to the 1960's," *Public Interest*, 25 (1971): 99-113.

TABLE 3. PRESIDENTIAL CHOICES OF THE
COLLEGE-EDUCATED (GALLUP)

<i>Years of College Attendance</i>	1948			
	<i>Dewey</i>	<i>Truman</i>	<i>Wallace^a</i>	<i>Thurmond^a</i>
1946-48 (N = 115)	47	41	9	2
1944-48 (N = 247)	53	37	8	1
1939-43 (N = 302)	57	34	5	3
1934-38 (N = 491)	56	34	7	1
1929-33 (N = 518)	64	29	3	3
1919-28 (N = 752)	70	25	2	3
1918 and earlier (N = 574)	69	27	1	2
All college-age cohorts (N = 2999)	62	30	4	3
Actual Presidential vote, total population (48,790,414)	45.1	49.6	2.4	2.4

<i>Years of College Attendance</i>	1956	
	<i>Eisenhower</i>	<i>Stevenson</i>
1954-56 (N = 40) ^c	70	30
1949-53 (N = 164)	55	44
1944-48 (N = 215)	59	41
1939-43 (N = 292)	66	34
1934-38 (N = 272)	64	35
1929-33 (N = 175)	55	45
1919-28 (N = 274)	75	25
1918 and earlier (N = 241)	75	24
All college-age cohorts (N = 1673)	62	38
Actual Presidential vote, total population (61,825,206)	57.4	42.1

<i>Years of College Attendance</i>	1964	
	<i>Goldwater</i>	<i>Johnson</i>
1962-64 (N = 159)	27	73
1956-61 (N = 310)	30	70
1950-55 (N = 330)	35	65
1944-49 (N = 321)	42	58
1939-43 (N = 367)	34	66
1934-38 (N = 307)	42	57
1929-33 (N = 191)	30	70
1919-28 (N = 271)	46	53
1918 and earlier (N = 109)	57	43
All college-age cohorts (N = 2365)	37	62
Actual Presidential vote, total population (70,420,910)	38.5	61.1

TABLE 3 (Continued)

<i>Years of College Attendance</i>	<i>Nixon</i>	<i>1968 Humphrey</i>	<i>Wallace^b</i>
1966-68 (N = 59)	41	48	12
1962-65 (N = 221)	45	42	12
1956-61 (N = 289)	51	36	13
1950-55 (N = 115)	57	31	11
1944-49 (N = 111)	60	32	8
1939-43 (N = 184)	56	30	13
1934-38 (N = 134)	56	35	9
1929-33 (N = 109)	47	44	8
1928 and earlier (N = 235)	67	22	11
All college-age cohorts (N = 1457)	54	33	12
Actual Presidential vote, total population (73,188,253)	43.4	42.7	13.5

^a Henry A. Wallace (Progressive); J. Strom Thurmond (States Rights)

^b George C. Wallace (American Independent)

^c N is too small for reliability.

Source: Seymour Martin Lipset and Everett Carl Ladd, Jr., "College Generations—from the 1930's to the 1960's," *Public Interest*, 25 (1971): 108. Copyright c. by National Affairs Inc., 1971.

among American college generations . . . has been toward a more liberal position over time,"⁴³ the unchanged loyalties of the older generations assign them a more conservative position on the political spectrum. But if this is the case how can one speak of "the moderating effects of growing older" except in the sense that the younger cohorts have grown less moderate?

According to Lipset and Ladd their table does suggest at least one striking instance of the permanent effects of the early political experience of a generation unit. They observe that the college generation of 1929-33, graduating into the "directionless gloom" of the Hoover years, would evince a permanent disproportionate antipathy toward

TABLE 4. REPUBLICAN PREFERENCE

<i>Years of College Attendance</i>	<i>Cohort</i>	<i>1948</i>	<i>1956</i>	<i>1964</i>	<i>1968</i>
44-48 (49)	A				
39-43	B	53			
34-38	C	57	59		
29-33	D	56	66	42	
		64	64	34	60
			55	42	56
				30	56
					47

⁴³ *Ibid.*, 113.

the Republican party. This disproportion does manifest itself in the elections of 1956, 1964, and 1968, but apparently the early trauma had not yet taken hold in 1948 when the Hoover generation voted substantially to the Right of its successors.

Lipset and Ladd would undoubtedly grant the tentativeness of any conclusions drawn from their limited data. But the conclusions they wish to draw, however tentative, put a burden on the evidence that it cannot support. The fact that younger college graduates are less likely to be Republican than are older cohorts is clear enough, but any persuasive statements about the effects of aging or early experience on the politics of various cohorts should be supported by more precise and detailed evidence, traced longitudinally through time.⁴⁴

Such evidence has recently been assembled by political scientists concerned with the statistical discrimination of the relationships between political behavior and age-specific variables. This literature has now accumulated to the point where it can support methodological controversies such as the running debate in the *Public Opinion Quarterly* over John Crittenden's article on aging and party affiliation.⁴⁵ I will briefly review the controversy, not for its substantive contributions, but for what it reveals about the problems of using longitudinal data to distinguish separate age-specific relationships.

Crittenden organized available survey data in a form that conveys the fluctuating political loyalties of age cohorts surveyed at four intervals between 1946 and 1958. On the basis of the data in table 5, Crittenden concludes that in non-Southern states, and irrespective of educational differences, aging was accompanied by an increase in Republican identity. He makes this point in two ways—first, by a vertical comparison of the age cohorts in each row, remarking that the percentages of Republican sympathizers in the older cohorts are uniformly higher than those in the younger cohorts. This of course tells us nothing about the political effects of aging but only about the political preferences of age groups in the given years. The burden of Crittenden's argument is therefore borne by his analysis and comparison of the changing preferences of successive cohorts as they age through the twelve years between 1946 and 1958. In order to fix on a measurement for the general direction of change in all the cohorts Crittenden decided to follow two cohorts from each age group through an eight-year period; for example, he records the shift in party prefer-

⁴⁴ For a nice example of the risks of generalizing on the basis of inadequate longitudinal data, see Norval O. Glenn and Richard E. Zody, "Cohort Analysis with National Survey Data," *Gerontologist*, 10 (1970): 237-40.

⁴⁵ John Crittenden, "Aging and Party Affiliation," *Public Opinion Quarterly*, 26 (1962): 648-57; Neal E. Cutler, "Generation, Maturation, and Party Affiliation," *ibid.*, 33 (1969-70): 582-88, followed by Crittenden's "Reply to Cutler" and Cutler's "Comment," *ibid.*, 589-92; Norval D. Glenn and Ted Hefner, "Further Evidence on Aging and Party Identification," *ibid.*, 36 (1972): 31-47.

TABLE 5. REPUBLICANS BY FOUR-YEAR AGE GROUPS
AND EDUCATIONAL LEVEL IN NON-SOUTHERN STATES

Age Group	1946		1950		1954		1958	
	Per Cent	(N)	Per Cent	(N)	Per Cent	(N)	Per Cent	(N)
High Education ^a								
21-24	46	(160)	41	(45)	42	(57)	43	(38)
25-28	54	(171)	43	(52)	45	(53)	51	(57)
29-32	51	(145)	44	(53)	39	(87)	49	(71)
33-36	59	(118)	50	(50)	47	(63)	49	(86)
37-40	59	(135)	53	(48)	51	(67)	42	(64)
41-44	70	(87)	58	(30)	56	(52)	44	(50)
45-48	58	(109)	58	(33)	52	(42)	34	(49)
49-52	58	(93)	50	(19)	89	(18)	62	(38)
53-56	60	(74)	60	(15)	66	(32)	47	(36)
57-60	65	(55)	75	(12)	58	(13)	63	(23)
61-64	58	(37)	86	(7)	75	(8)	55	(19)
65-68	70	(20)	60	(5)	90	(5)	66	(19)
21-68	57	(1204)	51	(369)	50	(497)	48	(550)
Low Education ^a								
21-24	36	(81)	14	(28)	38	(34)	41	(17)
25-28	52	(91)	18	(22)	37	(45)	24	(17)
29-32	43	(101)	26	(35)	29	(40)	38	(33)
33-36	42	(115)	32	(45)	27	(52)	30	(38)
37-40	51	(128)	28	(51)	38	(60)	33	(36)
41-44	40	(105)	40	(46)	36	(52)	39	(54)
45-48	54	(118)	44	(35)	38	(56)	34	(47)
49-52	44	(130)	48	(47)	39	(44)	37	(49)
53-56	59	(128)	37	(32)	45	(32)	46	(41)
57-60	59	(118)	42	(43)	50	(54)	47	(30)
61-64	58	(72)	61	(28)	50	(48)	50	(41)
65-68	52	(63)	58	(26)	59	(28)	45	(49)
21-68	49	(1250)	38	(438)	40	(545)	39	(452)

^a High education: graduated high school or better. Low education: did not graduate from high school. This basis of comparison is used in all subsequent tables employing education breakdowns.

Source: John Crittenden, "Aging and Party Affiliation," *Public Opinion Quarterly*, 26 (1962): 651.

ence of the cohort aged 21-24 in 1946 and 29-32 in 1954, and of the cohort aged 21-24 in 1950 and 29-32 in 1958. This gives him four entries for the age group, two in the High Education and two in the Low Education categories. Thus he finds, for the age group 21-24, three cohorts that shifted toward Republicanism and one that was unchanged, as

recorded in table 6. The raw figures do not give Crittenden the balance he finally strikes—an association of aging with increased Republicanism—until he corrects for the general tendency of the entire population, which is in the direction of the Democracy. That is, he counts an increase in Democratic sympathies smaller than that of the average of the entire population as a shift to relative Republicanism. The results of this method and of a similar treatment of Republican voting patterns produce his conclusion that “aging seems to produce a shift toward Republicanism in the period from 1946 to 1958.” Crittenden is quite tentative in suggesting reasons why this might have been the case as well as in granting that the effects of aging might have been complemented by “generational effects . . . that result from the impact of the Great Depression and New Deal.”⁴⁶ The modesty of his conclusions have not disarmed his critics.

In his article, “Generation, Maturation and Party Affiliation: A Cohort Analysis,” Neal E. Cutler uses Crittenden’s data as evidence for conclusions the reverse of those in the original article. Cutler correctly dismisses as beside the point Crittenden’s observation that the older groups were characteristically more Republican than the younger and proceeds to the main criticism, which is the inadequacy of the treatment of the longitudinal development of the cohort allegiances between 1946 and 1958. Cutler rearranges the data from Crittenden’s

TABLE 6. EIGHT-YEAR COHORT SHIFTS ON PARTY
IDENTIFICATION RELATIVE TO TREND

		<i>Time 2</i>		
<i>Age of Cohort</i>				
<i>Time 1</i>	<i>Time 2</i>	<i>More Republican</i>	<i>More Democratic</i>	<i>Same</i>
(21-24)	—(29-32)	3	0	1
(25-28)	—(33-36)	2	1	1
(29-32)	—(37-40)	4	0	0
(33-36)	—(41-44)	3	1	0
(37-40)	—(45-48)	1	2	1
(41-44)	—(49-52)	3	1	0
(45-48)	—(53-56)	2	1	1
(49-52)	—(57-60)	3	1	0
(53-56)	—(61-64)	2	1	1
(57-60)	—(65-68)	3	1	0
Total		26	9	5

Source: John Crittenden, *Public Opinion Quarterly*, 26 (1962): 652.

⁴⁶ Crittenden, “Aging and Party Affiliation,” 654, 657.

table on the political identification of the High Education population to demonstrate that in no instance in the twelve-year existence of a cohort can one find a linear increase in Republicanism (see table 7).

At first glance the controversy has something to do with "Fun with Numbers," or "How to Get Different Results from the Same Statistics." Cutler's emphasis on the issue of a linear pattern toward Republicanism is, however, justified by Crittenden's allegation of its existence. Cutler's longitudinal arrangement of the data shows that there was no regular progression toward increased Republicanism; his reading of the data even argues a progression in the opposite direction.

Cutler's arrangement of the data in table 8 reveals that whether one looks at changes in political allegiance recorded every four years, over an eight-year period, or across the entire span of twelve years, one finds that changes in a Democratic direction outnumber those in a Republican direction. Cutler also introduces calculations to show that, on average, fluctuations in political preferences are greater within "life stages" than within cohorts surveyed at four-year intervals. That is, there is less homogeneity across the age columns than along the cohort diagonals, "more homogeneity, associated with generational cohorts than with aging process or life-stage groups."

TABLE 7. AN EMPIRICAL EXAMPLE OF COHORT ANALYSIS^a

Age Intervals	Cohort ^b Labels	1946	1950	1954	1958	Lifestage ^b Labels
21-24	A					
25-28	B	46	41	42	43	(1)
29-32	C	54	43	45	51	(2)
33-36	D	51	44	39	49	(3)
37-40	E	59	50	47	49	(4)
41-44	F	59	53	51	42	(5)
45-48	G	70	58	56	44	(6)
49-52	H	58	58	52	34	(7)
53-56	I	58	50	89	62	(8)
57-60		60	60	66	47	(9)
61-64		65	75	58	63	(10)
65-68		58	86	75	55	(11)
Total		70	60	90	66	(12)
		57	51	50	48	

^a Cell entries are the percentage of each cell which identified with the Republican party in the year indicated. Source: John Crittenden, "Aging and Party Affiliation," *Public Opinion Quarterly*, 26 (1962): 651. Data represent the "high education" group in Crittenden's analysis.

^b Capital letters indicate the cohort diagonals; numbers in parentheses indicate life-stage rows.

Source: Neal E. Cutler, "Generation, Maturation, and Party Affiliation," *Public Opinion Quarterly*, 33, (1969-70): 585.

TABLE 8. ALTERNATIVE TESTS OF THE
AGING-REPUBLICANISM HYPOTHESIS^a

Cohort	Four-Year Differences			Eight-Year Differences		Twelve-Year Differences
	1946-50	1950-54	1954-58	1946-54	1950-58	1946-58
A:	—	—	+	—	+	+
B:	—	+	—	—	—	—
C:	—	+	—	*	—	—
D:	—	+	—	—	—	—
E:	—	—	+	—	+	+
F:	—	+	—	+	—	—
G:	—	+	—	+	+	+
H:	+	—	—	*	—	—
I:	+	*	—	+	—	+

^a A — indicates that the percentage of Republicans decreased from the first observation point to the second and fails to support the hypothesis; a + indicates that the percentage of Republicans increased, and supports the hypothesis; an * indicates no difference. Cell entries derived from table 1.

Source: Neal E. Cutler, *Public Opinion Quarterly*, 33 (1969-70): 586.

Crittenden erects various defenses of his method and conclusions, notably through his concept of correcting for trend, which identifies as relatively more Republican a shift in cohort opinion that is less Democratic than the average shift of the entire population. Cutler's answer to this is merely to remark that if the entire population is shifting away from Republicanism as it ages and if a majority of the cohorts are also shifting in that direction, it is rather odd to conclude that Republicanism increases with age.

In a recent re-examination of the controversy, Norval Glenn and Ted Hefner pose the issue in this way:

If the Crittenden data can be trusted, several important questions arise as to their proper interpretation. For instance, during a period in which the secular trend is away from Republicanism, is an increase in the "relative" Republicanism of an aging cohort evidence for a conservative influence of the aging process or of passage to the later stages of the life cycle? Or does it merely reflect a tendency for party identification in adult cohorts to remain stable?⁴⁷

Glenn and Hefner's assessment of revised and expanded survey data reveals a pattern of change that seems to confirm Crittenden's conclusions. They reject those conclusions, however, because the tendency for which Crittenden corrects—the trend of the entire population away from Republicanism—was significantly affected by the heavy

⁴⁷ Glenn and Hefner, "Further Evidence on Aging and Party Identification," 31.

mortality in the older, more Republican cohorts. Glenn and Hefner conjecture that mortality in the higher age brackets entails the disappearance of those least affected by the massive defections from the Republican party during the Great Depression, thus lessening the Republicanism of the entire population without contributing to a trend of the living population toward the Democrats.

The issue of correcting for trend has been re-examined in William R. Klecka's attempt to devise a statistical technique for leaching out the effects of all variations except those related to age—with fairly inconclusive results for the Crittenden controversy. Klecka has also proposed an alternative to the longitudinal analysis of arbitrarily defined uniform cohorts in his attempt to identify "empirically" the chronological dimensions of generations at a specific time and place. That is, he has attempted to construct a statistical device that will uncover the actual boundaries of generations by identifying significant changes in collective attitudes.⁴⁸ These are but examples of a substantial literature dedicated to compensating by statistical refinements for the limitations of available data.⁴⁹ Such efforts may prove suggestive to the few historians who struggle with the same sort of evidence, but in a broader sense, even the most arcane and least conclusive contributions of the social scientists do expose problems of generalization and inference that are implicit in most discussions of the relationship between age and collective behavior.⁵⁰

AMONG OTHER THINGS these contributions suggest what every good historian knows: that the way that evidence is selected and ordered is the way that questions are posed, and therefore the way that the possible answers are imposed. The very decision to examine arbitrarily defined age cohorts admits of some insights and excludes others. To

⁴⁸ William R. Klecka, "Some Strategies for Seeking Age Relationships in Political Behavior," a paper delivered at the annual meeting of the American Political Science Association, in September 1971. See also Klecka, "Applying Political Generations to the Study of Political Behavior: A Cohort Analysis," *Public Opinion Quarterly*, 35 (1971): 358-73.

⁴⁹ See for example, Gosta Carlsson and Katarina Karlsson, "Age, Cohorts and the Generation of Generations," *American Sociological Review*, 35 (1970): 710-18.

⁵⁰ Some of the fundamental issues are suggested in Glenn and Zody, "Cohort Analysis with National Survey Data," 233-40; see also, Richard E. Zody, "Cohort Analysis: Some Applicatory Problems in the Study of Social and Political Behavior," *Social Science Quarterly*, 50 (1969): 374-80, which deals with the problems of cohort overlap, sample attrition, and design asymmetry. The issue of controlling such variables as sex and education is explored in Norval D. Glenn and Michael Grimes, "Aging, Voting and Political Interest," *American Sociological Review*, 33 (1968): 563-75.

In addition to Klecka's paper cited in note 48 the following papers were delivered at the session of the annual meeting of the Political Science Association in 1971 devoted to research on the problem of generations: Neal E. Cutler, "Generational Analysis in Political Science"; Stephen J. Cutler, "Some Political Consequences of Prestige Loss Among the Aged"; Anne Foner, "Age Stratification and Ideological Cleavages"; T. Allen Lambert, "Generational Factors in Political-Cultural Consciousness."

select a particular statistical relationship is to choose a potential generalization. There was nothing in the logic of Crittenden's research that forced him to measure the political effects of aging by calculating cohort changes over eight-year periods. His even more basic decision to consider the "aging" that occurs between the ages of twenty-one and twenty-eight as functionally equivalent to the aging that occurs between the ages of fifty-seven and sixty-four imposed a range of possible answers to certain questions, but not to all possible questions, about the political effects of aging.

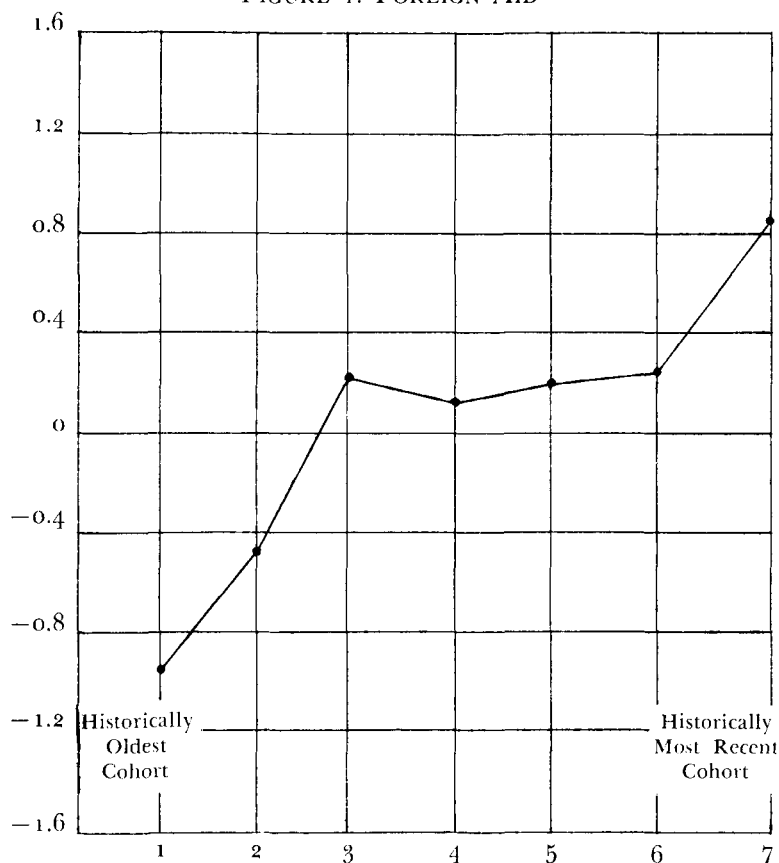
If we wish to compare how successive generations have viewed salient foreign policy issues, we might, as Neal Cutler did, construct a measure of cohort response to questions about foreign policy and then average this measure over the life of each cohort as it responded to four successive surveys administered between 1946 and 1966 (see figure 1).⁵¹ This would enable us to graph and thus compare age-related attitudes to foreign policy, expressed as an average response of the particular "generations," who are experiencing the same events at different stages of their life cycles.

Cutler interprets figure 1 to support the hypothesis that the younger the cohort the greater the support for foreign aid. The method of averaging does plausibly convey the existence of differences between age groups, not at a particular time but over time. However, the method conceals possible rhythms of difference and uniformity that might have been flattened out in the averages; and does not allow of questions regarding the effects of aging, for example, or the degree to which the collective opinion of a given cohort might have been affected by a traumatic event such as the Korean War. Cutler could have organized the evidence to bear on such questions but did not. There is no point in objecting to his choice but some in recognizing the way it shaped the possibilities of his conclusions.

In an even more basic sense the nature of the available evidence shapes the nature of the conclusions—for example, generational analyses based on survey data are limited by the inherent limitations of surveys. The most obvious of these has to do with time and place. Surveys successively applied so as to provide longitudinal data have been collected only for a short time, in a few places, and about a few topics. Social scientists recognize this, of course, but cannot always resist the temptation to draw, or at least suggest, large generalizations out of their narrow data base. Consider for example, Lipset and Ladd's modest, "In so far as we can generalize, Aristotle's emphasis on the moderating effects of growing older turns out to be more predictive

⁵¹ Neal E. Cutler, "Generational Succession as a Source of Foreign Policy Attitudes," *Journal of Peace Research*, 1 (1970): 33-47.

FIGURE 1. FOREIGN AID



Source: Neal E. Cutler, "Generational Succession as a Source of Foreign Policy Attitudes," *Journal of Peace Research*, 1 (1970): 40.

than Mannheim's theory of the long-term consequences of the early political experiences of 'generation-units.'"⁵² This dubious assumption of generality is not really saved by their disclaimer. There is no point to their guess that what they discovered about college-age cohorts in mid-century America "predicts" such generational patterns in other times and places and with regard to other forms of collective behavior—except to the extent that their findings falsify any formulation of the Mannheim theory as a universal law. Such gratuitous conjectures are usually controlled by the behaviorist super-ego; and a venerable tradition of criticism and self-criticism has instilled in survey practitioners a sophisticated sense of the conceptual limitations and practical flaws in extant survey data. Some of these, such as accumulated sampling error, systematic underrepresentation of certain social groups, changes in the wording of questionnaires applied in successive surveys, might be relevant to a particular generational study but are not germane to our general concerns.

⁵² Lipset and Ladd, "College Generations," 113.

One rather technical issue of survey research is, however, central both philosophically and practically to the systematic investigation of generations. This issue might be approached by discussing the difference between a "panel" and the sample of a cohort. When a panel is surveyed the same questions are periodically readministered to the same group of respondents. Longitudinal studies based on survey data do not, strictly speaking, measure the change in attitudes of the individuals originally surveyed, but the change in the proportion of those holding particular attitudes in successive samples of the same cohort.⁵³ A study that concludes that the surveyed cohort becomes more conservative with age is actually describing an increase in the percentage of those expressing conservative attitudes in the later samples, not the increasing conservatism of particular individuals surveyed in the early samples.

The relevance of this issue has to do with what William Evan calls "the biasing effect of changes in the composition of cohorts."⁵⁴ This would be no problem if one could assume that changes in the composition of cohorts through demographic loss, migration, and immigration were randomly distributed along the spectrum of attitudes surveyed. But in the world we precariously inhabit this is not always a safe assumption. There are not only demographic effects related to the normal erosion of the aged population or to the disproportionate erosion of males in the older cohorts, but also the immense age-specific destruction that accompanies war and other social tragedies. Thus unfortunately there is some sense in speaking of the virtual disappearance of an entire generation. One could not assume identity in the internal structure or the interrelationships of cohorts of French males surveyed in 1914 and 1919.

How such considerations might apply in less dramatic and obvious circumstances can be illustrated by a brief discussion of Maurice Zeitlin's treatment of political generations in his *Revolutionary Politics and the Cuban Working Class*.⁵⁵ Zeitlin's work is refreshing because it breaks out of the usual class and cultural boundaries of American

⁵³ For practical reasons the scope of panel studies is quite limited. One often-cited example is Erland N. P. Nelson, "Persistence of Attitudes of College Students Fourteen Years Later," *Psychological Monographs*, 68 (1954): 1-13. Although the time span and population examined are limited, the study reveals the care with which variables should be controlled to be able to draw any conclusions regarding collective shifts in opinion, even about the panel that was resurveyed.

⁵⁴ Evan, "Cohort Analysis of Survey Data," 72. Glenn and Zody (in "Cohort Analysis with National Survey Data," 239) argue that cohorts (that is, surveyed samples of a larger population) are preferable to panels "with a local or otherwise restricted sample in which changes in the sample cannot be related to changes in the total population." However, Nelson's study shows how, in principle at least, one might contrive controls for local deviance and for national trends. As I point out above, the relatively stable structure of recent American cohorts, which have no significant emigration or immigration, cannot be assumed for other times and other places.

⁵⁵ Maurice Zeitlin, *Revolutionary Politics and the Cuban Working Class* (New York, 1970), 211-41.

public-opinion research to investigate the political self-definition of Cuban working-class generations. He finds that successive age groups of Cuban workers retained the stamp of the historical circumstances prevailing at the time of their entrance into the labor force. Aging did not correlate with decreasing militancy. For example, those who entered the labor force from 1928 to 1935 during a period of militancy and Communist leadership remained, in 1962, significantly more sympathetic to the revolution and to communism than those some fifteen years their junior who received their political baptism during the 1940s under different circumstances.

Zeitlin's research was necessarily based on limited data. Such a problem as the adequacy of his sample is not our concern, but the force of his generational conclusions is weakened by certain other, probably insurmountable, limitations. Since he did not investigate age-related attitudinal shifts for the entire Cuban population, we cannot know the extent to which specific proletarian cohorts deviated from, or merely recapitulated, shifts of the larger population. An even more basic issue has to do with the stability of the sampled cohorts. Even if Zeitlin had been periodically able to survey samples of his cohorts during the entire era 1928-62 he could not have been certain that there was no relevant migration from the cohorts. This is not a completely abstract quibble because it is at least conceivable that the relatively negative response to communism in the age 36-43 cohort reflects the disproportionate erosion or emigration of those who had been Communist sympathizers in the 1940s.

I believe that these reservations qualify but do not vitiate the plausibility of Zeitlin's conclusions. And I certainly subscribe to his view that "failure to use the generational concept because its empirical demonstration is difficult is detrimental to the analysis of political behavior."⁵⁶ The commitment to the goal of empirical demonstration does, however, make some approaches to the problem of generations more plausible than others. The attempt to grasp the essence of the historical process through an analysis of age-specific relationships analogous to the Marxian analysis of class relationships has raised more problems than it can hope to solve. Thus we have not gotten very far with the elucidation of Mannheim's, "the phenomenon of generations is one of the basic factors contributing to the genesis of the dynamic of historical development,"⁵⁷ but we might well settle for the elucidation of specific historical phenomena, of the sort suggested by Mannheim.⁵⁸

⁵⁶ *Ibid.*, 240.

⁵⁷ Mannheim, *Essays on the Sociology of Knowledge*, 320.

⁵⁸ See *ibid.*, 290 n: "It is a matter for historical and sociological research to discover at what stage in its development, and under what conditions, a class becomes class-conscious, and

I HAVE ARGUED ABOVE that so-called generational phenomena have to do with age-specific relationships that may or may not matter; that such relationships vary in nature; and that the varieties must be distinguished in order to decide how statements about them might be verified. I have identified the following distinct, though sometimes overlapping, categories in which age-linked differences might constitute significant historical variables:

(1) Recurrent collective behavior is associated with a certain phase of the life cycle. This conception of the "ages of man" has most often been applied to behavior peculiar to youth, or to the presumed effects of aging.

(2) Groups of coevals are stamped by some collective experience that permanently distinguishes them from other age groups as they move through time. Social scientists usually characterize this as a generational as opposed to a life-cycle effect.

(3) Different groups of coevals may simultaneously experience the same significant events but respond to them in distinct ways more closely associated with age than with other variables.

(4) Particular circumstances produce extraordinary temporary differences between age groups, constituting a generation gap. A gap that regularly recurs, however, would actually be a phenomenon of stages in the life cycle, as in the first category described above.

(5) Attributes separating a cohort from older age groups may persist in the behavior of all subsequent cohorts. Then what began as a generational difference eventually characterizes the entire population under a certain age.

(6) Changes in the relative size of cohorts may cause significant temporary or permanent differences, linked to age such as those that result from large age-specific demographic losses.

I believe, without insisting on it, that the other distinctions mentioned in this article can be made to fit more or less comfortably into one of the above categories. They are presented, not as a rigid taxonomy, but to suggest that useful generalizations proceed from appropriate distinctions.

similarly, when individual members of a generation become conscious of their common situation and make this consciousness the basis of their group solidarity."

Statesmen Undisguised

A Review Article by ROY M. MACLEOD

RAY JONES. *The Nineteenth-Century Foreign Office: An Administrative History*. With a preface by W. N. MEDLICOTT. (London School of Economics and Political Science. L.S.E. Research Monographs, Number 9.) London: Weidenfeld and Nicolson. 1971. Pp. 224. £3.75.

WILLIAM C. LUBENOW. *The Politics of Government Growth: Early Victorian Attitudes toward State Intervention, 1833-1848*. (Library of Politics and Society.) [Hamden, Conn.:] Archon Books. 1971. Pp. 237. \$10.00.

HENRY ROSEVEARE. *The Treasury: The Evolution of a British Institution*. [New York:] Columbia University Press. 1969. Pp. 406. \$12.50.

MAURICE WRIGHT. *Treasury Control of the Civil Service, 1854-1874*. New York: Oxford University Press. 1969. Pp. xxxv, 406. \$13.00.

GEOFFREY KINGDON FRY. *Statesmen in Disguise: The Changing Role of the Administrative Class of the British Home Civil Service, 1853-1966*. New York: Humanities Press. 1969. Pp. 479. \$18.50.

HENRY PARRIS. *Constitutional Bureaucracy: The Development of British Central Administration since the Eighteenth Century*. (Minerva Series of Students' Handbooks, Number 23.) New York: Augustus M. Kelley. 1969. Pp. 324. \$11.50.

GILLIAN SUTHERLAND, editor. *Studies in the Growth of Nineteenth-Century Government*. Totowa, N. J.: Rowman and Littlefield. 1972. Pp. viii, 295. \$12.50.

IT HAS BEEN fifteen years since the appearance of Professor Oliver MacDonagh's article on the "Nineteenth-Century Revolution in Government."¹ In retrospect, and even at that time, it was clear that something of a revolution was then under way in British administrative historiography. By the

¹O. O. G. M. MacDonagh, "The Nineteenth-Century Revolution in Government: A Re-appraisal," *Historical Journal*, 1 (1958): 56-67. See also his "Delegated Legislation and Administrative Discretions in the 1850s," *Victorian Studies*, 2 (1958): 44-60. Professor MacDonagh's argument is amplified and illustrated in his book, *A Pattern of Government Growth: The Passenger Acts and Their Enforcement, 1800-1860* (London, 1961).

mid-1950s the making of the postwar "welfare state" had quickened a tremendous impetus among British social and economic historians to study and describe the long and winding administrative and legislative paths that had led in the direction of the new and, to preserve the metaphor, "revolutionary" social services. The transformation of British society during and after the war had not ushered in quite the millennium of which social planners had dreamed; it had been, after all, what W. H. Beveridge had called an "English revolution"—silent, or relatively so, and full of compromise. But the processes by which it had begun in the nineteenth century were seen as historically vital. Historians turned to the lessons of administrative history with a new sense of discovery.²

With hindsight, this development was highly important to the study of administrative history itself. Traditionally administrative history in Britain had been more or less a subdivision of constitutional history, usually concerned, following Tout and Maitland, with the elaboration of constitutional law or with the legislative chronology by which new departments emerged and old ones expanded or changed.³ There was, in addition, a genre of memoirs, of "amateur" history in the best sense, including reminiscences of old Whitehall hands, which recounted for younger and amazed generations of hard-worked civil servants those gentler days when the administrative class, like the fountains in Trafalgar Square, regularly played from ten to four.

Although social historians had examined the great extension of legislation and administration in the nineteenth century in the context of economic and social developments, most administrative historians of Victorian Britain looked for guidance to Professor A. V. Dicey's classic *Lectures on the Rela-*

² See R. M. Titmuss, *Essays on the Welfare State* (London, 1958); Asa Briggs, "The Welfare State in Historical Perspective," *Archives Europeenes de Sociologie*, 2 (1961): 223; Dorothy Wedderburn, "Facts and Theories of the Welfare State," *The Socialist Register, 1965* (London, 1965), 127-46; T. H. Marshall, "Citizenship and Social Class," in *Sociology at the Crossroads and Other Essays* (London, 1963), 67-127; John Saville, "The Welfare State: An Historical Appraisal," *New Reasoner*, 3 (1957-58): 3-25; Dorothy Thompson, "The Welfare State: Discussion," *New Reasoner*, 4 (1958-59): 125-30; see also David Roberts, *Victorian Origins of the British Welfare State* (New Haven, 1960); Maurice Bruce, *The Coming of the Welfare State* (London, 1961); Calvin Woodard, "Reality and Social Reform: The Transition from Laissez-faire to the Welfare State," *Yale Law Journal*, 72 (1962): 286-328.

³ See *The Collected Papers of Thomas Frederick Tout*, ed. F. M. Powicke (Manchester, 1932), and F. W. Maitland, *The Constitutional History of England* (new ed.; Cambridge, 1961). See also the early and the "new" Whitehall series, well-known examples of which include Sir Edward Troup, *The Home Office* (London, 1925); Sir Arthur Newsholme, *The Ministry of Health* (London, 1925); Sir Thomas Heath, *The Treasury* (London, 1927); and Lord Bridges, *The Treasury* (London, 1969). The study of administrative history in Britain owes, in addition, an especial debt to the editors of the quarterly journal, *Public Administration*. See also Robert Moses, *The Civil Service of Great Britain* (New York, 1914); Sir Adair More, "Officials and Policy," *Public Administration*, 5 (1927): 461-70; Emmeline W. Cohen, *The Growth of the British Civil Service* (London, 1941); H. R. C. Greaves, *The Civil Service in the Changing State* (London, 1947); K. B. Smellie, *A Hundred Years of English Government* (London, 1950); and M. W. Thomas, "The Origins of Administrative Centralisation," *Current Legal Problems*, 3 (1950): 214-35.

tion between Law and Public Opinion in England during the Nineteenth Century, first delivered at Harvard in 1898 and published in England in 1905. Dicey tried to outline constitutional developments as mediating critical shifts in law, political theory, and public opinion. His emphasis, however, was placed chiefly on the legislative process rather than on specifically administrative forms, and his intention was to describe trends or movements in ideas, with their corresponding political and administrative characteristics. He outlined in broad terms a highly generalized description of the transition he perceived within British administration from a period in which assumptions were apparently based on laissez faire individualism to a period apparently dominated by "collectivism." Each period, in his view, was distinguished by a particular climate of opinion, derived from an influential thinker or set of ideas. Thus, the period from about 1800 to 1830 he described as one of "Old Toryism" and "legislative quiescence," in which Paley's natural philosophy and Blackstone's legal philosophy combined to endorse and maintain traditional social and political institutions. In Dicey's scheme this period gave way to a period of "individualism" influenced by Bentham and his followers and characterized by legislation designed to extend "personal liberty," political rights, and civil freedoms. This was the "heyday of *laissez-faire*" and of Utilitarianism, when a small intellectual elite within the civil service acquired the status, in James Stephen's phrase, of "statesmen in disguise." The third phase, between 1865 and 1900, saw, in Dicey's terms, a final transition between individualism and collectivism, in which the civil service expanded, the range of state responsibilities were extended, and faith was increasingly placed in the benefits to be derived from state intervention.⁴

For nearly half a century Dicey's interpretation and its assumptions dominated administrative history and the history of social policy. Indeed Dicey's interpretation had an elegant simplicity to recommend it, and for many it neatly summarized received opinion. By the late 1950s, however, new work began to reveal certain flaws in its argument.⁵ Dicey clearly recognized the importance of economic and social interests in the making of policy. But his emphasis upon the "Rule of Law"⁶ and the work of the Benthamites in influencing legislative opinion led to the neglect of other

⁴ A. V. Dicey, *Lectures on the Relation between Law and Public Opinion in England during the Nineteenth Century* (London, 1905). see especially page 22. The influence of Dicey's example, cast against the context of social policy, was brilliantly demonstrated in the series of lectures delivered at the London School of Economics in 1957-58 and published in Morris Ginsberg, ed., *Law and Opinion in England in the 20th Century* (London, 1959). See also G. Kitson Clark, "'Statesmen in Disguise': Reflections on the History of the Neutrality of the Civil Service," *Historical Journal*, 2 (1959): 19-39.

⁵ See especially Roger Prouty, *The Transformation of the Board of Trade, 1830-55: A Study of Administrative Reorganisation in the Heyday of Laissez-faire* (London, 1957), and R. J. Lambert, *Sir John Simon, 1816-1904, and English Social Administration* (London, 1963).

⁶ See Dicey's introduction to the second (1914) edition of *Lectures on the Relation between Law and Public Opinion*, and see also W. Ivor Jennings, "In Praise of Dicey, 1885-1935," *Public Administration*, 13 (1935): 123-34.

factors activating government in particular areas. Eventually Dicey's explanations were seen to beg crucial questions about the changing form and function of state activity, to ignore the influence of technological innovation and new forms of expertise, to oversimplify the tremendous transformation occurring within the civil service, and to leave unexplained the manner by which departmental and political interests actually contributed to the making of policy.

The first sustained critique of Dicey's work appeared in 1948 in an article by J. B. Brebner. Brebner demonstrated difficulties on several levels: for example, Dicey had incorrectly believed Benthamism to be synonymous with the principles of laissez faire and therefore opposed to the concept of state intervention. Professor Lionel Robbins has since reminded us that if one's reading of Bentham is confined to certain chapters of his works, it is indeed possible to credit Bentham with an extremely negative view of the state. But Henry Fawcett long ago argued that it was not Bentham but the free-trade movement, the Anti-Corn Law agitation, and the writings of Harriet Martineau that fixed laissez faire in the British mind. Furthermore Élie Halévy recognized that Bentham's philosophy embodied both a rationalist philosophy of state intervention in restraint of disorder and civil justice and a postulate of individualism founded on the belief that all individuals in society have an approximately equal capacity for happiness. Halévy added that "Benthamism was the work of a jurist who was by accident an economist."⁷

Brebner saw further that there is a basic fallacy in believing Benthamism to be a monolithic, coherent structure. In the Benthamite legal system the expediency of any act of government must be tested by its consequences for the greatest happiness of the greatest number. In this system, as Robbins explained,

the invisible hand which guides men to promote ends which were no part of their original intention is not the hand of some god or natural agency independent of human effort, it is the hand of the law-giver, the hand which withdraws from the sphere of the pursuit of self-interest those possibilities which do not harmonise with the public good.⁸

⁷ J. B. Brebner, "Laissez-faire and State Intervention in Nineteenth-Century Britain," *Journal of Economic History*, 8 (1948): supp., pp. 59-73. Dicey believed that "though laissez-faire is not an essential part of Utilitarianism it was practically the most vital part of Bentham's legislative doctrine and in England gave to the movement for the reform of the law both its power and its character." *Lectures on the Relation between Law and Public Opinion*, 147. See, too, Lionel Robbins, *The Theory of Economic Policy in English Classical Political Economy* (London, 1952), 39. Henry Fawcett, who believed that a government with powers of intervention was like a physician with a dangerous drug, and who deprecated state provision to those who saw it as a "right," nevertheless explicitly stressed that there was "nothing whatever in the principles of economic science to lead to the establishment of any general conclusion with regard to the advantages or disadvantages of State interference." "The General Aspects of State Intervention," in Henry Fawcett and M. G. Fawcett, *Essays and Lectures on Social and Political Subjects* (London, 1872), 33. Finally, see Élie Halévy, *The Growth of Philosophic Radicalism* (London, 1952), 488.

⁸ Robbins, *Theory of Economic Policy*, 56.

Accordingly, an acceptance of state intervention in certain circumstances was as important to Benthamism as the doctrine of *laissez faire*; both elements were inextricably connected by the empirical requirements of a pragmatic utility.

Because Dicey largely ignored this interventionist element, it became possible, even easy, to find instances of state activity between the 1830s and the 1860s where the notion of a governing principle of *laissez faire* apparently did not apply.⁹ We also know now that many classical economists did not espouse the principle of *laissez faire* as necessarily or invariably the best instrument of social policy.¹⁰ In his introductory lecture at University College, London, J. E. Cairnes suggested that *laissez faire* was

at best a mere handy rule of practice, useful perhaps as a reminder to statesmen on which side the presumption lies in questions of industrial legislation, but totally destitute of all scientific authority. . . . It must never for a moment be allowed to stand in the way of the candid consideration of any promising proposal of social or industrial reform.¹¹

In view of the accumulating evidence of government growth from 1830 onwards, Dicey's conception of the "heyday of *laissez-faire*" has not unnaturally lost some of its hold on the work of administrative historians. Nevertheless there were in the late 1950s a very few who chose to ask questions that probed the relationships among political philosophy, pragmatic political demands, new technological requirements, and internal administrative development in the context of the growth of Victorian government. Oliver MacDonagh was one of these few. His article in the *Historical Journal* in 1958 had an immediate effect. In substance MacDonagh set out to discover

what men thought, and what men felt, contemporary practices should be . . . ; what external or overt events directed the current of affairs decisively, or made men fully conscious of the tendencies of their time; what the underlying social and economic pressures and the medical, engineering, and mechanical potentialities consisted in; and what was actually taking place within executive government itself.

⁹ For an illuminating discussion of *laissez faire* as myth and metaphor, see H. Scott Gordon, "The Ideology of *Laissez-Faire*," in A. W. Coats, ed., *The Classical Economists and Economic Policy* (London, 1971), 180–205.

¹⁰ Thus J. R. McCulloch: "The Principle of *laissez-faire* may be safely trusted to in some things but in many more it is wholly inapplicable; and to appeal to it on all occasions savours more of the policy of a parrot than of a statesman or a philosopher." *Treatise on the Succession to Property Vacant by Death* (London, 1849), 156. Later in the century Professor J. E. Cairnes wrote that he was "unable to find in the maxims of abstract justice any key to the practical problems of the distribution of wealth," but that this did not justify, *ipso facto*, the arguments of *laissez faire*. "The recognition of private property and freedom of individual industry" must, he thought, "be judged by practical utility." *Some Leading Principles of Political Economy* (London, 1874), 320.

¹¹ Cairnes, "Political Economy and *Laissez-faire*," in his *Essays in Political Economy* (London, 1873), 244, 251.

He suggested first, that government did indeed grow in the 1850s and 1860s, but that we must look deeply into government departments to see clearly how this occurred; second, that Benthamites had little influence on most policy in this period and certainly less than historians had previously assumed; and third, that a conceptual model could be devised to describe the process of administrative growth that took place in the middle quarters of the century.¹²

"MacDonagh's model," as it was subsequently called, attempted to relate the political, economic, and social circumstances of the early decades of the century to the reforms in government emerging after the Reform Bill of 1832, the Northcote-Trevelyan report, and the Crimean War.¹³ His work aroused some explosive opposition from those who saw the claims of ideas, opinion, and especially Benthamism being written out of history,¹⁴ and the forces of an internal logic of legislation and administration rising in their place. This debate continues, but few can doubt that in his attempt to reach beyond traditional constitutional history MacDonagh precipitated much useful discussion about the explanatory validity of "models" and "phases" in historical writing¹⁵ and about the analytical value of legislative and administrative evidence. This discussion occurred at about the time when many young British historians were beginning to experience the heady wines of sociological and economic analysis, borne by the new Bacchae of the social sciences. Even when the historiographical debate in MacDonagh's wake seemed to spend more time upon the semantics of description than upon matters of evidence or interpretation, the subject commanded lively

¹² MacDonagh, "Nineteenth-Century Revolution in Government," 54, *passim*. The importance of these questions was stressed in S. E. Finer, *The Life and Times of Sir Edwin Chadwick* (London, 1952), and has since been repeatedly argued by G. Kitson Clark, *The Making of Victorian England* (London, 1962); Lambert, *Simon*; and W. L. Burn, *The Age of Equipoise* (London, 1964).

¹³ MacDonagh's model was based on five stages of administrative change and, though not intended to correspond in detail to particular cases, was based largely on his earlier work on the regulation of emigrant traffic and was intended to provide a suggestive way of viewing other examples of government intervention in this period. The first stage described the exposure of a social evil and the passage of initial, often permissive legislation to remedy it. When such legislation was found inadequate or ineffective, special officers were appointed to enforce it, and a second stage ensued. A third stage appeared when the special officers themselves began to respond to the needs of their posts by pressing for further legislation and for a superintending central body. In the fourth stage a dynamic conception of administration replaced a static one, as administrators ceased to regard their problems as soluble simply by the addition of more legislation and additional staff. This gave way to a fifth stage, when the officers took upon themselves a wider ambit of administrative discretion and the administration itself became merged with the body of government.

¹⁴ Among the earliest criticisms of MacDonagh's model was that by Henry Parris, "The Nineteenth-Century Revolution in Government: A Reappraisal Reappraised," *Historical Journal*, 3 (1960): 17-37. See also Jennifer Hart, "Nineteenth-Century Social Reform: A Tory Interpretation of History," *Past and Present*, no. 31 (1965): 39-61. For evidence of the extent in which this "Benthamite vs. anti-Benthamite controversy" had developed by the mid-sixties, see Robert Gutchen's review of Lambert's *Simon* in *Victorian Studies*, 8 (1964): 82-83.

¹⁵ An excellent summary of the debate and its limitations appeared in Valerie Cromwell, "Interpretations of Nineteenth-Century Administration: An Analysis," *Victorian Studies*, 9 (1966): 245-55.

attention. In the university context alone, administrative history appeared to come out of a deep sleep and at last seemed something one could literally discuss with one's students, rather than a dusty, quasi-theological canon that was dutifully learned by rote for the routine catechism of finals.¹⁶

But this was just the beginning. Over the next decade MacDonagh's article helped to focus attention on what he called "self-generating bureaucratic growth" and stimulated many historians to consider seriously what the expression "administrative growth" really means. A new world of metaphor opened before us. In what sense, for example, is administration properly described as an "organic" phenomenon, historically capable of analysis in terms of "behavior," "motivation," and the influence of competing pressures? Alternatively, to what extent is administration, in Lord Haldane's classic expression, best considered in terms of the "machinery of government"—a complicated set of mechanical instruments constructed by constitutional clockmakers working to principles of nearly Newtonian universality, with logical internal requirements for satisfactory performance and with a delicate balance easily upset by sharp changes in political temperature and pressure? And where in one's administrative historiography does one include the influence of political philosophies or of religious, social, or economic pressures? And what of the civil servant and the politician, and the none-too-clear relationship between them? Where must the administrative historian place his emphasis?

Such were some, at least, of the questions that had emerged by the mid-1960s. It was then already clear that the range of detailed studies was still too narrow to generate even what R. K. Merton might call "second-order generalizations." We had excellent work by a small group of scholars including G. S. R. Kitson Clark, Jennifer Hart, Henry Parris, R. J. Lambert, and S. E. Finer, but the range of central—let alone local—government activities amenable to study was enormous. In any case, it was by no means clear that the mere accumulation of new examples would lead inductively to a new or in some sense objectively more accurate picture of government growth. Indeed, the more detailed studies that emerged, the more one began to feel that MacDonagh's model, far from being a finished blueprint (which he, of course, would not have claimed) was little more than a builder's sketch. By the late 1960s the graduate students in Britain and abroad were given the task of articulating this new historiography. The study of different government departments and of different aspects of policy began to reveal the rich variety and density of the subject. Students were set to unraveling the intricate connections between law and lawmaking opinion, between

¹⁶ One is tempted to draw a parallel with the impact of another "revolutionary" study—Thomas S. Kuhn, *The Structure of Scientific Revolutions* (New York, 1962)—upon the intellectual assumptions of a different field. It is perhaps not too much to suggest that MacDonagh's work, like Kuhn's, helped precipitate a "paradigm shift" in theoretical areas of problem choice and interpretation.

social change and civil-service reform, between expertise and administration, and between departmental policies and party politics.

Broadly speaking the books under review reflect this new industry. On the one hand, there has been a substantial monographic literature arising (some would say too quickly) from Ph.D. theses;¹⁷ subsequently we have had a number of extended biographies, articles, and books, often themselves building upon graduate theses and reflecting additional years of hard work at the rock face of records in Chancery Lane. More recently there have appeared general textbooks that summarize recent work and render it more accessible to historians and students of public administration. Finally, thanks to the efforts of Dr. Gillian Sutherland, a colloquium, organized under the auspices of *Past and Present* and attracting sixty people, has resulted in a series of extremely valuable essays.¹⁸ This evidence—all most welcome—of serious interest in the subject provides a convenient opportunity for an interim stocktaking of the work in hand.

INTO THE CATEGORY of recent monographs come the works of Dr. Ray Jones and Dr. W. S. Lubenow. Dr. Jones's study of the Foreign Office, the first historical volume in the LSE Research Monograph series, follows developments in that department through the eyes of the staff, from the administrative reforms of 1848 to the "Great Reforms" of 1906. Despite its obvious political importance, the Foreign Office has held few charms for administrative historians. It has the reputation of being intellectually isolated and mildly reactionary, stuffy and crotchety; even its accommodation—in "dark offices and labyrinthine passages, four houses at least tumbled into one, with floors at uneven levels and wearying corkscrew stairs that men cursed as they climbed"—gives the impression of inertia, meanness, and indifference to public criticism. It is not surprising to learn that the Foreign Office was the last department to which administrative innovations were applied. To its credit, its officers could claim particular success in standing up to the Treasury—a novel virtue, one might think, important to preserve at all costs—but this was hardly to their credit if the costs were too great. Probably they were. Dr. Jones well describes successive attempts to develop principles of administrative integration and devolution and concludes with an informed discussion of the Foreign and Diplomatic Services.

A difficulty arises, however, when one closes the book and asks oneself what one has learned from Dr. Jones about the *business* of the Foreign Office. What effect, for example, did administrative change have on depart-

¹⁷ It is a revealing sign of the growing professionalism of administrative history that five of the seven books under review owe their origins to graduate theses.

¹⁸ See Gillian Sutherland, "Reform of the English Civil Service, 1780-1914: A Project for a Colloquium," *Past and Present*, no. 42 (1969): 163-65; Gillian Sutherland, "Recent Trends in Administrative History," *Victorian Studies*, 13 (1970): 408-11.

mental policy and objectives? This leads to a larger question: can administrative history at its best be confined solely to structure and organization and formal rules of competition? What of the old risk of isolating form from content, structure from substance? What of considerations of morale, administrative leadership, and bureaucratic innovation? What of the political attitudes of civil servants themselves? One recalls the anti-German (and to a lesser extent, anti-French) bias of the higher permanent staff and Sir Warren Fisher's famous attempt to place an anti-German in the post of permanent secretary at the Foreign Office in 1939. Given that the duty of the first division (now the administrative class) in the last analysis was (and is) to advise its ministers, such questions are not trivial. Dr. Jones's study shows an excellent grasp of technical matters, but it should be read in conjunction with Dr. Zara Steiner's *The Foreign Office and Foreign Policy, 1898-1914* (1969), with which it will inevitably be compared. One must read both together to come away with the sense that the Foreign Office was, after all, staffed by living beings and that, however removed from the lime-light of national or international political events, the substance of their views was critical to the efficient execution of business and perhaps to the direction of policy.

Dr. Lubenow's book draws upon a broader canvass. Looking above the tempest of day-to-day administrative issues, Dr. Lubenow discusses what he believes to be influential political attitudes toward government growth in Britain between 1833 and 1848. He has made a thoughtful and in many ways courageous attempt to grasp the elusive explanations of intellectual influence that Dicey failed to cement and to propose a view of governmental growth that incorporates different models of political behavior. Dr. Lubenow's attitude toward the "great debate"—"resurrecting old and outworn controversies for antiquarian purposes"—is perhaps unfair, given that this assumption is by no means universally shared. A more significant problem in this work is its use of a strange meta-language of political analysis, making it sometimes difficult for the reader to perceive beneath its surreal imagery the plain, detailed landscape of British history. For example, Dr. Lubenow's discussion of what he describes as the "attitudinal pattern of Victorian Britain" is weakened by the absence of an attempt to prove the prior assumption—that there is such a thing as an "attitudinal pattern," that Victorian Britain (however defined) possessed it, and that in some way it had a mysterious power no less disturbing than Banquo's ghost. Dr. Lubenow also tends to reify the notions of "thought" and "opinion" and to consider them as variables somehow independent of the people who actually held them. The difficulties inherent in making tangible the elusive elements of *Zeitgeist*—including that most difficult of all legacies from Dicey, the "spirit" of lawmaking opinion—are not resolved; they are merely replaced by newer sociological concepts of equal turbidity.

A similar difficulty emerges when we try to use what Dr. Lubenow de-

scribes as two possible models of Victorian political behavior: the so-called organic view of politics and the so-called incrementalist view. Dr. Lubenow appears to argue that the incrementalist view provides a *via media* between the interpretation of government growth as either a "carefully prepared blueprint for political change" or a "consequence of a comprehensive and exhaustive policy analysis." But as neither alternative interpretation appears very probable, and as no one to my knowledge has ever proposed either, the problem (and thus the solution) seems false. One is inclined to credit Dr. Lubenow's thesis that Victorian government growth did not lead ineluctably to a welfare state and that there was much less opposition along theoretical lines than Dicey might have supposed; but neither thesis is strengthened much by shaky references to a notion of incrementalist growth.

In his study Dr. Lubenow relies heavily upon blue books and parliamentary debates to support his conclusion that Victorian attitudes toward state intervention were conditioned by the "structure and assumptions of traditional political forms" rather than by "socialist attempts to create a welfare state." One might question on methodological grounds whether the study of traditional political sources could really lead to any other conclusion. One is led to suppose, and not only from his bibliography, that Dr. Lubenow has not made use of unpublished departmental files or correspondence. But surely the real difficulty is the way in which the argument is set out: we do not come away from this book much wiser about the nature or the significance of intellectual beliefs or public demands in the formation of policy.

Within the category of more extensive studies fall two excellent treatises on the Treasury and Treasury control, by Dr. Henry Roseveare and Dr. Maurice Wright. As James Stansfeld, financial secretary in Gladstone's first government, once said, the Treasury is a "useful department to be in; it gives you the clue to most others." Dr. Roseveare's work, already widely praised, gives us several clues to the writing of good administrative history. His book is an extremely successful example of an approach that blends the study of administration with the study of policy and casts both in a fresh light. Dr. Roseveare accepts the premise that a history of the Treasury, properly conceived, "could hardly fall short of being . . . a social, an economic, and a political history of Britain." In his hands the Treasury, as an institution, comes alive—born of the specific needs of a medieval monarchy; its growth dictated, first by royal convenience, then parliamentary influence; its stature sometimes eclipsed, sometimes strengthened by its political leaders and its permanent heads; its functions slowly, even passively, changing in war and peace, from an eighteenth-century ideal of a "balanced constitution" to the Keynesian and post-Keynesian assumptions of a "managed economy."

Dr. Roseveare describes in rich detail the growing functions of the Treasury—the drafting of legislation, the administration of revenue, the overall

control of expenditure, and ultimately the management of the civil service. At the same time he sees the weaknesses—personal, administrative, and political—that by the mid-nineteenth century rendered the Treasury impotent to place more than a temporary brake on the growing expenditure of departmental government. Ironically it was largely this crippling impotence, reflected in its apparent preoccupation with the parsimony of “candle-ends” accountancy, that shaped the Treasury’s public image. Yet it was chiefly the sense of innate moral superiority that comes with a balanced budget, a sense of property and propriety, and an ability to shift fiscal needs from political expedience that gave the Treasury its special strength. Moreover, fostered from the late seventeenth century by such men as the eponymous Sir George Downing (an early gift of Harvard College to British financial history), the Treasury established a hallmark in administrative order and elegance that set precedents for all Whitehall. Yet from time to time, and vividly by 1900, the weight of its moral pride in “Lingenism” and in monetary orthodoxy as well as the sheer intellectual aloofness of its senior clerks left the heavily armored Treasury moored outside the mainstream of radical innovation in the range and effectiveness of civil government. To political and social reformers the Treasury, and particularly the bulwarks of Treasury control, stood as an obstacle instead of a defense. With the new century and the sharp transformations of two world wars came new generations of civil servants and a new recognition that responsibility for public expenditure must be a “managing” responsibility. The Treasury’s twin functions of finance and management were by then inseparable, but they had to pull in harness in response to changing political perceptions of public demands. Dr. Roseveare, with a becoming modesty that would profit the Treasury itself, traces its growing responsibility through its several reorganizations, its dealings with the Bank of England and the National Economic Development Council, up to the dark, ambiguous days of George Brown (now Lord George-Brown) and the ill-fated Department of Economic Affairs.

It is only perhaps in his concluding pages, devoted to the Fulton Commission’s report, that Dr. Roseveare rises with a defending vengeance to argue the Treasury’s side against the alleged tendency to promise “change for change’s sake,” which he suggests will inevitably follow Fulton. Dr. Roseveare concludes, however, that “time’s argument in all this will prove more interesting than any historian’s.” He is still right. Today, five years after Fulton, the Treasury seems to have weathered the storms of criticism. One cannot leave the vast Edwardian monument in Great George Street without a grudging admiration for its sheer tenacity.

If Dr. Roseveare has woven a broad tapestry, Dr. Wright provides much necessary needlework. His purpose is to suggest that the role of chief villain in the story of nineteenth-century administration is not by rights the Treasury’s. Restricting himself to the years between the Northcote-Trevelyan

reforms and the Playfair Commission, he probes deeply into the questions of organization, control of establishments, conditions of service, and Treasury control. His accomplishment is solid, giving vital details concerning the "department of departments" in cross-sections of crystalline clarity. No student of Victorian administrative history can afford to neglect the sheer substance of establishment debates; the endless difficulties of salary, office hours, and discipline; the interminable negotiations with different departments and different political chiefs over the implementation of open competition; and the conduct of Treasury committees of inquiry.

In Dr. Wright's opinion the traditional view of the Treasury as financial watchdog has been greatly oversimplified. He argues convincingly that if the Treasury behaved in a niggardly way in its formal dealings, this was merely what Parliament, indeed the public, asked of it. Even in this the Treasury was not wholly successful, as pleas for improved salaries or increased departmental provision were often granted, if only after much emotional bloodshed. Retrenchment proved a good slogan but an elusive reality. Likewise, we are told, the Treasury was not completely hidebound by its rules and precedents; indeed it frequently found expediency more attractive than principle. Ministers (for example, A. S. Ayrton) who persisted in injurious cheeseparing were sometimes sacked.

Dr. Wright suggests that the Treasury was not really hardhearted; perhaps it was, at worst, the villain with the heart of gold. Besides, other Treasury activity provided a flexible and cheap device for conducting government business. This was certainly true of the system of Treasury committees. Moreover, the Treasury's attempts after 1854 to combine economy and efficiency, to rationalize the service, and to eradicate "abuse," jobbery, and favoritism were in principle laudable. "Beneficence combined with flexibility" might have been a Treasury code, and the avoidance of dispute in the interests of conciliation was to be its distinguishing characteristic. It must be said, however, to anyone looking at the Treasury from outside, particularly from the so-called second-class departments (including the Board of Trade, the Local Government Board, the Board of Agriculture, and the Science and Art Department), in which so much of the country's new work of government was taking place, the Treasury's mandarinism seemed remarkably imperious. Dr. Wright provides an exemplary defense, but surely the case cannot be said to be complete without hearing the views of the plaintiffs.

This one-sidedness is perhaps the one weakness underlying Dr. Wright's otherwise solid edifice. It does not, however, diminish his achievement to suggest that the book loses by omitting vital mention of the eternal and detailed cut and thrust between the Treasury and the departments and the sense of conflict that demoralized so much of Victorian public service. Administrative historians know too well how negotiating often degenerated into haggling, where "conciliation" was a euphemism for weary acquiescence,

where "beneficence" was a charade for polite bludgeoning, and where assertions of "administrative flexibility" could be mere rationalizations of defeat.

These issues, fundamental to the business of administrative history, remain when one turns to Dr. Fry's book. This vast product of an enlarged Ph.D. thesis begins with a "general argument" of three hundred words, a section that, for many, may replace the reading of the rest of the volume. Dr. Fry's work is not limited to historical analysis, even though his book will doubtless form a useful contribution to the historiography of the subject. His chief mission is to use historical arguments to support certain related contentions: that the regulatory state of the nineteenth century has now been succeeded by a welfare state; that modern government in the welfare state rightly places a premium on management, specialized knowledge, and research; that the acceptability of the so-called all-rounder in the administrative class is now much diminished; that in the interests of better government the administrative class and the executive class should be merged into one; and that greater mobility among staff between all sections of government should be encouraged.

Dr. Fry devotes only his first chapter to the history of the civil service before 1900 and spends the remainder of his time discussing the background and shortcomings of civil-service recruitment and training and the neglect shown the "specialists." In so doing he makes much thorough use of secondary literature and published government reports. But as his main concern is to recommend change in the organization of government as it affects the service today, he does not give great space to specific issues of legislative or administrative control or to personal leadership within the existing system. Indeed there is much about his book that will dissatisfy historians: his level of generality, his assumption of a high order of homogeneity in a system that thrives (in spite of the Treasury) upon its diversity, and his neglect of the "fine structure" of internal governmental debate that can only come with the detailed analysis of specific departments or decisions or acts of Parliament.

Yet Dr. Fry's discussion does raise several important questions. To what extent, for example, was the typical mid-Victorian civil servant really an all-rounder? Which departments in the last half of the nineteenth century could really function without a good measure of hardheaded specialist advice of a legal, technical, or financial nature? How important were the notorious watertight barriers between departments and within individual departments in contriving a series of pragmatic specializations? How deleterious were these barriers to the work of individual departments? How much more innovation came about through the use of patronage and specialist appointments, both permitted to a significant extent throughout Whitehall until well after 1870?

That Dr. Fry is on the side of the angels of the moment is evident from his obvious preference for the specialist over the all-rounder, from his

disagreement with the position taken by Lord Bridges, and from his views on the Fulton report, to the criticism of which he devotes a huge postscript of nearly sixty pages. But mere assertion will not exorcise the ghosts of traditionalism from the administrative class, nor is it a foregone conclusion that the British civil service, for all its manifold deficiencies, will be renewed to grace by the straightforward acceptance of Dr. Fry's prescriptions.

As a textbook welcome to both students and administrators, Dr. Henry Parris's recent *Constitutional Bureaucracy* is a particularly valuable contribution to the literature. Dr. Parris's early participation in the "nineteenth-century revolution" debate is well known, and his study of *Government and the Railways in Nineteenth-Century Britain* (1965) has enjoyed a wide readership. His present book, clearly the result of almost a decade of scholarship (his own, no less than others) devotes most of its space to the discussion of specific issues in the rise of a "constitutional bureaucracy" as a counterpart to constitutional monarchy. These issues, including patronage, ministerial power and responsibility, and the "grey eminence" myth, are set against the relationship among legislation and administration, Treasury control, and the relevance of public administration to our conception of public accountability and "representative bureaucracy." Dr. Parris's book is a careful distillation, concisely written. His dissection of Dicey, Halévy, and MacDonagh in chapter nine is extremely serviceable. One only wishes he could have devoted more space in this chapter to reviewing the ramifications of the old debate, particularly the relationship between law and opinion, the character of this relationship in a given period, the nature of laissez faire, the rationale of legislation in health, transport, industrial regulation, and education, and the important differences existing within "old" and "new" departments of state. If Dr. Parris's conclusions stand out in measured similarity to his views of 1965, they do leave the debate much more accessible. One is led to see that the old nettle of collectivism was rather a red herring—an *idée fixe* in Dicey's interpretation of social history. In fact, state intervention and laissez faire were not necessarily incompatible; on the contrary, they were contingently related, and in practice individuals could reasonably espouse both.¹⁹ All depended on the circumstances of the case.

We turn finally to the recent work edited by Dr. Gillian Sutherland. It is a pleasure to welcome to the growing canon of new scholarship these ten essays that reflect an extremely animated sense of historical enterprise. Important generalizations that emerge from the essays again revolve round the questions that have informed the whole discussion—the role of political ideas in government growth (ideas versus pragmatism), civil-service reform and the strong personalities of different departments, the role of "zealots"

¹⁹ See Gordon, "Ideology of Laissez-Faire."

and experts as innovators and as agents of legislative change, and the interpretative view of politics and the state that all this implies.

To the first question, both Professor Samuel Finer (whose paper alone did not originate at the *Past and Present* colloquium) and Dr. Alan Ryan address themselves precisely. Professor Finer's immensely provocative use of scientific imagery to describe the process by which the political ideas of Bentham, aided by men of influence, were projected into society—a three-fold process, as he calls it, of “irradiation, suscitation and permeation”—has the immediate appeal of elegant simplicity. Dr. Ryan's study of J. S. Mill's views on the concepts of sound knowledge and good government provides an extremely useful introduction to the context of administration in India and Britain and to the transfer of administrative experiments from one of Britain's largest “social laboratories.”²⁰ Ryan, like Finer, argues for the importance of political theory in the causation of legislative change, but, unlike Finer, he does not insist upon basing his analysis on a single like-minded group of Utilitarians, possessed of a system of homogeneous beliefs. Instead, in his view legislative change often followed the charge of an “intellectual cavalry” riding a generalized set of intellectual assumptions that could help bring about administrative change by helping to instill moral changes in the perception of administrative objectives. While their conclusions may be debated, both Finer and Ryan will become required reading. So far it is clear that Benthamism and the particular impact of Utilitarian radicalism up to 1850 will continue to be the point at issue. It will be interesting to see how the argument will develop when intellectual undercurrents of lawmaking opinion in the last half of the nineteenth century—what Melvin Richter calls the “politics of conscience”²¹—come under the microscope.

With the essays of Jennifer Hart and A. D. Donajgradski on the Northcote-Trevelyan report we think ourselves on familiar ground until we are gently led to question conventional assumptions about the effective nature of patronage (including its class connotations) and the real purpose of financial reform. From Mrs. Hart's point of view civil-service reform was fundamentally an internal development following with inexorable logic from the combined desires of Trevelyan and Gladstone for efficiency and economy and for a “purer and more strenuous ethic” in public life. Although writing from a different point of view Donajgradski, in his case study of the Home Office in its reactions to the Northcote-Trevelyan report, concludes with Mrs. Hart that the real contest was between patronage and efficiency and between the supposed negative values of the one and the virtues of the other. Donajgradski goes rather further toward destroying the mystique of the Northcote-Trevelyan report by disclosing the im-

²⁰ As W. L. Burn once tantalizingly described Ireland. “Free Trade in Land: An Aspect of the Irish Question,” *Transactions of the Royal Historical Society*, 4th ser., 31 (1949): 68.

²¹ See Melvin Richter, *The Politics of Conscience: T. H. Green and His Age* (London, 1964).

balance of its extremely biased arguments. We have, of course, long been released from any reason to believe in blue books as impartial collections of value-free observations and balanced reportage, but it is good to be reminded not only of the value-laden character of the Northcote-Trevelyan report but also of its consequences. In the light of Donajgradski's conclusion that the report "failed to describe the old system accurately . . . defined non-existent problems and proposed new clerical roles of doubtful utility," our view of it will never be the same again.

The remaining essays by Dr. Sutherland and Dr. Richard Johnson on education, by Dr. Snelling and Dr. T. J. Barron on the Colonial Office, by Mrs. Valerie Cromwell and Dr. Zara Steiner on the Foreign Office, and by Dr. Roger Davidson on the Board of Trade all demonstrate careful interest both in the internal dynamics of departments in relation to social and political demands and in the notion of departmental "personalities." In a reflective introduction Dr. Sutherland concludes that state action, and consequent government growth, was virtually always seen, even by those whom Henry Parris calls zealots, in a relatively negative light, in the sense that the state was urged to act only when attempts at local, voluntary, and individual action had failed. In a comparative European context there were few ideological collectivists in British government in the period from 1870 to 1914, and the starting point for state intervention was clearly remote from the centralized assumptions of France and Germany. It could be valuable to compare Britain with France and Germany in this period to discover how external influences of humanitarianism or science could combine with internal departmental and parliamentary concern for efficiency and economy, or with the more straightforward concerns of politicians to be re-elected, in order to produce a dramatic "interventionist" effect on a representative political system that prided itself on its individualism.

THESE SEVEN BOOKS give us a sense of the current preoccupations and expectations of administrative history. The nature of the research program is becoming clearer, and several outstanding tasks await the historian. First, it is generally agreed that studies of government departments, as such, cannot wholly succeed unless they grasp both the details of office and the objectives for which those offices were created—that is, policies and machinery must be assessed together if we are really to make sense of either, and both must be seen, as Valerie Cromwell has reminded us, against a larger background of political maneuver.²²

It follows that the notion of administrative history as beginning where the "interesting" history of political turmoil ends can now be safely buried.

²² Valerie Cromwell, "Interpretations of Nineteenth-Century Administration."

As Royston Lambert once observed, historians have repeatedly made the mistake of assuming that once a piece of legislation was on the statute books the precedent was created and the rest was mere routine. We now know that it is profoundly wrong to confuse the passage of an act with the actual execution of policy. Jennifer Hart makes the same point in a different way when she reminds us that it was a mistake to see in the Northcote-Trevelyan report pressures that existed only after the report appeared.

It is also certain that interest in administration will encourage new work on both the history and the mechanics of legislation. In this direction one of Professor MacDonagh's most valuable contributions was to describe not merely legislation but directed sequences of legislation, called forth in what may become quite explicable patterns, moving with the aid of reformers' opinion to specific enabling legislation, then with administrative opinion to tighter or more general enabling legislation and the appointment of inspectors, then to compulsory legislation, consolidation, devolution to local authorities, or even repeal. Future work should consider different kinds of general and special legislation and the relationship between certain legislative forms and political objectives. Some acts, we know, were intended to re-establish a status quo that the influence of changing circumstances had perversely distorted. They consisted, to paraphrase Henry Buckle, "not in doing something new, but in undoing something old." Such legislation attempted to "restore things to the natural channel from which the ignorance of preceding legislation has driven them." This attitude, for example, governed the assumption of the *Times* in 1869 that

people may, in a general way, be trusted to govern themselves and manage their own affairs, and that a State can do little more than render some formal and official assistance, chiefly in the removal of obstruction. There are evils that impede the current of affairs and stop the circulation. The State can do little more than clear the thoroughfare and bid all to "move on."²³

In contrast other acts illustrated new legislative assumptions, some of which embodied the explicit extensions of government responsibilities for technical reasons or for reasons of social and economic justice. In the one case the presence of the state was felt desirable where private initiative had not sufficed; in the second the state was asked to extend standards of hygiene and education into areas where previous action had no effect.

Such fresh administrative and legislative studies will unavoidably rest on a more thorough knowledge of departmental history. As Dr. Roseveare's work demonstrates, the study of administrative history need not be obsessed with the artifacts of government—the "seemingly inert materials" of writs, seals, and minute books. These inert materials can be brought

²³ Henry Buckle, *History of Civilisation in Ireland* (London, 1872), 1: 375; *Times* (London), Aug. 10, 1869.

to life when the faceless image of administration is given flesh and color. We know now that, whatever the formal position may have been, certain civil servants and their associates could play an important role in "innovational bureaucracy." It remains for future research to show us how innovations could be encouraged, or resisted, and how their success bore upon the goals of the department.

In this context much remains to be learned about the reception of expertise. Much has been written about the eternal contest between those two famous ideal types—the generalist (or all-rounder) and the specialist (or "professional")—the one educated in law or classics and inclined to keep things running smoothly, the other keen with enthusiasm for righting wrongs and possessing the knowledge with which to do so. This conflict did exist, as the histories of Edwin Chadwick, John Simon, and Angus Smith and the vivid stories of scientists in the War Office and the Admiralty amply recall. But historians must not be deceived into thinking that such a polarization always occurred or that there was, *pace* Dr. Fry, always a clear division of opinion about the merits of either caste. The picture is much more complicated. For example, there are distinct generational phenomena to be taken into account. Within the civil service, patterns of administrative behavior changed decisively between 1870 and the turn of the century, and the growth and differentiation of the service sometimes militated for and sometimes against the rapid acceptance of new ideas. Again, to take another example, one finds that the "heroic age" of a particular species of expert could be quickly succeeded by a period of apparent quiescence and consolidation.²⁴ Experts, as Michel Crozier once observed, may well only remain heroic as long as they are on the leading edge of policy.²⁵ Once their advice is taken and assimilated into the system, many become merely another part of the system, defending their own group or professional interests. This seems to have taken place among the lawyers, the doctors, the engineers, and, at least since the Second World War, the scientists. Who is to say that it is not now also occurring to our most modern zealots, the economists and the planners?

It will be interesting, therefore, to explore the conditions of the period from 1870 to 1914 during which generalists and experts in most departments had begun to reach a *modus vivendi*. Possibly one will see, as Richard Johnson argues, the "zealous expert" giving way to the "administrative expert." One has increasing evidence that by the middle of the nineteenth century, specialist technical knowledge (and even research) was becoming accepted as a prior condition of action, and that, conversely, departments

²⁴ See Roy M. MacLeod, "The Alkali Acts Administration, 1863-1884: The Emergence of the Civil Scientist," *Victorian Studies*, 9 (1965): 85-112; "Social Administration and the 'Floating Population': The Canal Boat Acts, 1877-1899," *Past and Present*, no. 35 (1966): 101-32; and "Government and Resource Conservation: The Salmon Act Administration, 1861-1886," *Journal of British Studies*, 7 (1968): 114-50.

²⁵ Michel Crozier, *The Bureaucratic Phenomenon*, tr. by the author (London, 1964), 165.

realized (and advised their ministers) that legislation could proceed no faster than the science of a particular subject permitted. By the turn of the century there were signs that scientific and specialist expertise, within limits, was becoming part of the accepted orthodoxy. This, in turn, began to raise new problems of parity for the experts and even greater problems of ensuring a steady flow of the best experts from the new universities into Whitehall. But what were the limitations placed on government policy by the state of knowledge? And what became of the zealots?

Such questions remain to worry us. And no less important are the questions still posed by the rubric of "Treasury control." Whether and how the Treasury in fact "controlled" expenditure is now better known, thanks to the work of Dr. Wright and Dr. Roseveare. We know that the application and principles of control were by no means uniform from department to department, that they were often no more than delaying devices, and that they could be overruled by personal intervention at ministerial level. One important goal of future research must be to determine how, in practice, the Treasury reacted to requests of different kinds, what role "personalities" and expertise played in these decisions, and whether the watertight divisions of the Treasury militated against any concept of a broad view of public spending. We especially need a book that will build upon the excellent work of Roseveare and Wright by taking specific issues, in the hands of specific Treasury clerks, through the corridors of specific spending departments, together with detailed vertical studies of individual departments and individual issues of policy and administration. At the same time we need works that will, for specific historical periods, take wider, interdepartmental views across Whitehall and raise issues in a political and economic perspective.

Within this contextual framework we might also give continuing attention to two related issues—the circumstantial characteristics of government activity and the relationship between individual officials (whether generalist or specialist), their political masters, and the public they are meant to serve. A. P. Thornton has recently observed that "ideas in politics, as elsewhere, are forced to fight a grinding battle with circumstance,"²⁶ and surely no student of the British civil service would deny that the climate in a department has a profound influence on the ways in which policies are executed. Moreover, within the firmament of departments each sphere of government has its own distinctive Pythagorean harmony. Complicated questions of morale, access to the "top," prospects for promotion, intra-departmental tensions and rivalries, and a sense of hierarchy and status are fundamental to an understanding of a department's overall attitude toward its work. It is insufficient to assume that single, universal, and straightforward definitions exist in practice for such concepts as patronage,

²⁶ A. P. Thornton, *The Imperial Idea and Its Enemies* (London, 1959), ix.

competition, accountability, and responsibility. We know, for example, that patronage was not an unmitigated evil but a flexible, pragmatic tool that could be turned to good effect, whether in the appointment of specialist advisers, or, as Richard Johnson puts it, at the "oldest and highest" strata of administration.

Finally, it is unequivocally clear that no study of the effect of ideas in administration can wholly succeed without considering the actual relationship between permanent officials, their own beliefs, and their political leaders. By the late nineteenth century, permanent secretaries were notoriously self-possessed and wielded considerable power. That they could, and still can, aid or attenuate the policies of even the most strong-willed politician is today common knowledge. But how was this done, and with what effect? This has a timely interest, particularly outside academic circles. Can there be, for example, substance in the belief that in extreme cases the permanent civil servant can thwart the political programs of Number 10 Downing Street, as Mrs. Marcia Williams has recently asserted? On the other hand, has the civil servant become more vulnerable to public criticism through the development of the select-committee system or through such innovations as the Central Policy Review Staff? As Lewis Gunn has written, the notion of "Ministerial responsibility rarely involves the acceptance of culpability by the Minister and . . . even Ministerial answerability is sometimes so diluted as to offer little protection for the anonymity of the official."²⁷

During the last century the role of the civil servant has become increasingly visible. At the same time public willingness to accept without question his use of power has been significantly weakened. The character of governmental accountability, through Parliament to the people, is now one of the pressing issues before the British public. Since the Fulton report this issue has been extended to the civil service, whose senior members are now recognized virtually as "statesmen undisguised." This appreciation has given administrative history a new relevance, far removed from the old-fashioned notion of constitutional chronology. Perhaps we are now beginning to perceive that administrative history is not merely about administrative development and structure per se but about the objectives and priorities of public interest and government policy. Indeed can one now see administrative history as a fundamental discipline, helping to interpret structures in the light of achievements, wedding social policy and social administration, and providing a framework for analyzing the social and political basis of power and responsibility in our society.

²⁷ Marcia Williams, *Inside No. 10* (London, 1972), 344-57; Lewis Gunn, "Politicians and Officials: Who is Answerable?" *Political Quarterly*, 43 (1972): 253-60.

De Gaulle as President: First Triumphs and Last Memoirs

A Review Article by JOHN C. CAIRNS

ANDRÉ MALRAUX. *Les chênes qu'on abat . . .* [Paris:] Gallimard. 1971. Pp. 235. 21 fr.

CHARLES DE GAULLE. *Mémoires d'espoir*. Volume 1, *Le renouveau, 1958-1962*; volume 2, *L'effort, 1962 . . .* [Paris:] Plon. 1970; 1971. Pp. 314; 223.

CHARLES DE GAULLE. *Memoirs of Hope: Renewal and Endeavor*. Translated by TERENCE KILMARTIN. New York: Simon and Schuster. 1971. Pp. 392. \$10.00.

PHILIPPE ALEXANDRE. *The Duel: De Gaulle and Pompidou*. Translated by ELAINE P. HALPERIN. Boston: Houghton Mifflin Company. 1972. Pp. xvii, 360. \$7.95.

MAURICE COUVE DE MURVILLE. *Une politique étrangère, 1958-1969*. [Paris:] Plon. 1971. Pp. 499.

"BETWEEN OURSELVES, Malraux, when you get right down to it, is it worth the trouble?" the General asked his visitor one winter day in the final year of his life. "Why write?"¹ Was he serious? Did he merely wish to savor again the delicious historical ruminations of his "brilliant friend, the devotee of lofty destinies,"² the oracle of the Gaullist mystique, who, like some world-weary Alexander of the intellect, seemed to have accomplished the meeting of East and West, and who never failed to locate de Gaulle and his epic in the rhythmic progression of great men and creative moments in the history of the world? Did he wish again to hear a discourse on de Gaulle and Caesar as men of action and of letters, on the implications for a hesitant writer at Colombey of the *Mémorial de Sainte Hélène*? If it was not merely a rare entertainment in a quiet country house

¹ Malraux, *Les chênes*, 59; the translation, *Fallen Oaks, Conversation with de Gaulle* (London, 1972), is not without rough and awkward renditions—a very unsatisfactory substitute.

² De Gaulle, *Mémoires d'espoir*, 1: 285; translated as *Memoirs of Hope: Renewal and Endeavor* (New York, 1971).

lying in the snow-covered fields of Champagne, was it a reinforcement of purpose that he anticipated, pitched at the universal level?

De Gaulle left office in April 1969; the first volume of his second installment of memoirs was in his editor's hands by July 1970.³ There could not have been much hesitation. He had remarked of the earlier war memoirs that, unlike Churchill who had "just strung things together," he wished to leave "a literary work."⁴ It is fair to say that he did it again, although his oft-repeated query whether God would spare him to complete his task was answered negatively. Whether there is a falling off from the standard of the first memoirs is a matter of opinion. The framework for action was, all in all, less dramatic, and the documentation here is thinner. But in many respects it is the mixture same as before, an intensely subjective reading of a four-year period much written about and still only sketchily known. The two volumes, of which the second, *L'effort, 1962 . . .*, is no more than a fragment appended to the first, *Le renouveau, 1958-1962*, take de Gaulle from the moment of his return to power in 1958 to the 1962 alteration of the constitution by popular referendum. This he was unable to achieve until he had secured his position by ending the war for Algerian independence, destroying the army's political temptations, and overwhelming the opposition of the political parties. It was then that they finally understood that for de Gaulle Algeria was no more than the opening act in the work of reconstructing the state and society. In a sense, then, the manuscript breaks off at the pinnacle of his achievement, what he thought of as the triumph of "hope." "That is also why," he told Malraux, "I'm far from preparing the second volume (let's not mention the third!) with the same feeling."⁵

Time had both altered him and left him much the same. "He has, of course, aged a lot," Harold Macmillan noted in June 1958. "He has grown rather fat; his eyes are bad and he wears thick spectacles. . . . His manner is calm, affable and rather paternal. But underneath this new exterior, I should judge that he is as obstinate as ever."⁶ The pose now adopted in the memoirs is again that of the beleaguered hero, opposed by the jealous pygmies of sclerotic institutions (rather than the giants of the vanished war era), supported by the people whenever they can break through to make their will known. Of the celebrated *traversée du désert*, 1946-58, with all its frustrations, interventions, embittered silences, and philippic predictions of the wrath to come, little is said here. The Fourth Republic is written off in fourteen pages as a kind

³ Jacques Chapus, *Mourir à Colombey* (Paris, 1971), 224.

⁴ J.-R. Tournoux, *La Tragédie du général* (Paris, 1967), 233.

⁵ Malraux, *Les chênes*, 192-93.

⁶ Harold Macmillan, diary, June 29-30, 1958, in Macmillan, *Memoirs* (London, 1966-72), 4: 448.

of mad comedy in which powerless French heads of state presided over a nonstop political ballet, with only its industrial progress, owing to de Gaulle's own initiatives before the 1946 resignation, worth commending. The unfortunate *le Rassemblement du Peuple Français* experience is dismissed summarily as a victim of the malevolence of the parties, the government, the unions, business, and the press. Thus the crisis of May 1958 is reached, for obvious reasons, at a gallop. Jean Monnet once remarked of de Gaulle that "he has an odd technique. He always creates problems in order to solve them."⁷ This may not be entirely fair, but the memorialist unwittingly suggests that there is some truth in the proposition.

Grave problems there certainly were. Whether he expected to return to power remains an enigma now as before.⁸ His position is that he had nothing whatever to do with the emerging crisis in Algiers and Paris during the spring of 1958, though he realized at once that he must take hold as "l'instrument désigné" in the face of the collapse of "what people through force of habit still called the government." Superbly he recounts how he summoned to his house the prefect of the Haute-Marne and ordered him to tell Prime Minister Pierre Pflimlin at once that "the public interest demanded that he see me." No explanation is offered for the May 27 statement he issued, saying he had begun the regular procedure to establish a government (Pflimlin had not given up; the president, René Coty, had charged him with no such mission). His account of the meeting, May 28, with the *présidents* of the two chambers overlooks his refusal to present himself to the National Assembly. He does not recall telling Guy Mollet, during the May 30 conversion at Colombey, that because of poor health he no longer sought power.⁹ There is no mention of his having been kept informed of events in Algiers during those days, or of General Salan sending General Dulac to apprise de Gaulle of the Algiers plans, though it is alleged that de Gaulle criticized the dispositions and told Dulac to do what was necessary if parliament and the government continued blocking the way to his return.¹⁰ This recollection of the opening round, when Coty and the government yielded, de Gaulle compromised, and the parachute troops failed to

⁷ Jean Monnet, Dec. 15, 1959, quoted in C. L. Sulzberger, *The Last of the Giants* (New York, 1970), 625.

⁸ When General Paul Ely asked him in June 1958, "At what moment, General, did you come to feel that your return to power was certain?", meaning at what moment between May 13 and the investiture by parliament, de Gaulle turned away abruptly, saying "Always!" Paul Ely, *Mémoires* (Paris, 1964-69), 2: 342. But he told Macmillan in March 1960 that "he never expected to return to power—at least not in recent years." Macmillan, *Memoirs*, 5: 183.

⁹ De Gaulle, *Mémoires*, 1: 21-25, 28, 29-32; cf. André le Troquer, *La Parole est à André Le Troquer* (Paris, 1962), 181-99.

¹⁰ Jacques Soustelle, *L'Espérance trahie* (Paris, 1962), 45-46; Soustelle, *Vingt-huit ans de gaullisme* (Paris, 1968), 145-47.

appear, is something less than a full statement. What is missing is less some terrible admission of complicity in a plot than a frank explanation of his own situation within the overall crisis of the state. The scenario is spare. Prizes and punishments are handed out: commendations to Pflimlin or Coty, reprimands to Pierre Mendès-France or Jacques Duclos. Even the shade of Edouard Herriot is not spared a school-masterish rebuke for a foolish attempt to read de Gaulle a lecture on the Resistance (Herriot's record in that epic was somewhat muted) twelve years earlier, and the General recalls "the rather blunt and ironical reply it deserved" and he apparently delivered, with a slight suggestion of relish.¹¹

After the blameless return (in response to "the contract which the France of past, present and future imposed on me eighteen years before") came the resolute determination to end the war through a grant of independence to Algeria. Various he says that a wiser Fourth Republic doubtless could, or perhaps might, have enabled an autonomous Algeria to evolve peacefully into an association with France,¹² but he is almost silent on the record of the Provisional Government before that (and on the inhuman reprisals inflicted following the May 8, 1945, independence demonstrations and killing of some twenty-nine Europeans in Kabylia). His private conversations during the *traversée du désert* suggest that he had come to see independence as both desirable and inevitable. Speaking of Morocco, he once remarked to General Catroux: "If I were the government of France, I should not let independence be wrested from me, I should bestow it." His problem was how to create the requisite conditions for Algeria. He admits calculatedly "flinging" to the crowd in the Algiers forum the notorious ambiguity, "Je vous ai compris," with apparent spontaneity, one of the first of many tactical feints. But he reveals little. He claims to have measured General Salan's "slippery and enigmatic" personality (he owed his return in some measure to this man) from the first and to have determined to be rid of him soon, though this was not clear at the time.¹³ He recalls a town clerk in some Kabylia village taking him aside to expose the fraudulent popular enthusiasm and warn that the people wanted independence, not France. He confesses that his prime minister, Michel Debré (with the new 1958 constitution in force de Gaulle had been elected president of the Republic), both acted with

¹¹ De Gaulle, *Mémoires*, 1: 33; cf. Tournoux, *Tragédie*, 196, 205.

¹² *Ibid.*, 34, 17-18, 49.

¹³ Tournoux, *Tragédie*, 241-44; cf. de Gaulle, *Mémoires*, 1: 50, 57. Olivier Guichard, then de Gaulle's directeur du cabinet, said, however, that "he was not able to persuade de Gaulle on this mistrust [of Salan]. Among de Gaulle's faults is that he thinks he can regulate military affairs by handing out promotions and decorations. He is simply unaware of the fact that he, de Gaulle, never made the slightest imprint on the French army." Mar. 1, 1962, quoted in Sulzberger, *Last of the Giants*, 853.

complete loyalty and vented his hostility to self-determination.¹⁴ He remembers commanders who pledged their fidelity to him and withdrew it in April 1961.

Of that revolt staged in Algiers by the four generals known for months to be plotting against him, he says he feared a paratroop descent on Paris; but he passes in silence over the famous panicky appeal to the population launched by Debré as a consequence of de Gaulle's silence and (before the ringing television and radio denunciation of the quadrumvirate which broke them) apparent paralysis that had unnerved the cabinet.¹⁵ Faithful to his formula, he asserts both that he had "the clear-sighted support of the people" and that few voices supported him, whether he was trying to extricate France from Algeria or defend her garrison at Bizerte against President Habib Bourguiba's fell assault. Nearly all were arrayed against him, whether Dag Hammarskjöld, the OAS ("the scum of the military mob"¹⁶), unnamed foreigners,¹⁷ or even his second prime minister, Georges Pompidou, who, with the minister of justice, Jean Foyer, intervened and maneuvered to prevent General Jouhaud's being executed—the drama of which encounter is missing here, though the remembered bitterness may be read between the lines.¹⁸

The loss of Algeria is presented as inevitable in the circumstances; its ultimate negotiation into independence was the result of an almost singlehanded turning of "a page of our history." Perhaps de Gaulle was never imperially minded: his military education had oriented him more obviously toward Europe than toward the colonial world. But it remains uncertain from these memoirs at what point he came to believe that the European empires were doomed. Though he invokes the Brazzaville Conference of January 1944, he gave no indication at that time. Somewhat curiously, he recalls Ferhat Abbas approaching him in those days to say that de Gaulle then, more than anyone else before or after, could bring about a democratic Algerian state federated with

¹⁴ Debré, as Christian Fouchet put it, was "too visibly unhappy with the way things were working out," and Soustelle remarked on his "curious masochism." Fouchet, *Mémoires* (Paris, 1971), 1: 146; Soustelle, *L'Espérance trahie*, 97.

¹⁵ De Gaulle, *Mémoires*, 1: 112; Alexandre, *The Duel*, 72-73.

¹⁶ De Gaulle, *Mémoires*, 1: 128.

¹⁷ With some justice: for instance, Ambassador James M. Gavin told Sulzberger on January 15, 1962, that "our only policy is to stick with de Gaulle until he achieves an Algerian settlement and then to drop him like a hot potato." See Sulzberger, *Last of the Giants*, 839. Georges Pompidou even claimed that there had been a "serious plot" to oust de Gaulle two years earlier. American money was said to have been linked with Alain de Sérigny and others. The inference was that the CIA was behind it. The scheme was said to have involved a government that would include Georges Bidault and the election of Antoine Pinay to the presidency. See the conversation with Sulzberger, Feb. 11, 1960, *Last of the Giants*, 641.

¹⁸ De Gaulle, *Mémoires*, 1: 135; cf. Alexandre, *The Duel*, 86-92; Tournoux, *Tragédie*, 413-18. Significantly, de Gaulle does not name Fouchet as being in the opposition camp, though in fact he advised the president against carrying out the execution. No doubt his fidelity to the last earned him this silence. See Fouchet, *Mémoires*, 1: 172-79.

France. "But," he says, "given that in the middle of a war, in the frightful situation in which our country then found itself, there were more imperative and urgent matters than that one to be resolved, I had listened to Ferhat Abbas with as much reticence as interest."¹⁹ The explanation is unconvincing; it is essential to the suggestion—it is no more than that—that had he been in power after January 1946 the process of imperial devolution in North Africa and Asia might have been different.²⁰ Things did not turn out that way; to him fell the ungrateful task of ending "the colonial epic" after others had produced only war, defeat, and civil strife.

Of the evolution of black Africa and Madagascar to independence he has relatively little to say. It caused small fuss. But the uncooperative Guineans are shown as being already enveloped in a totalitarian system by that first summer of 1958: even the women were dressed alike, singing and dancing on cue. "Young, vibrant and ambitious," Sékou Touré, for his insolence, was read a sharp lecture, and as de Gaulle departed he said to him, anticipating a vote to leave the empire, "Farewell, Guinea!" Those who remained with France, to achieve independence within interdependence over the following two years, are favorably presented, each leader, whether Houphouët-Boigny, Senghor, or M'Ba, receiving a brief but special word of commendation. And on the smooth transition with which this enormous change was effected a note of self-congratulation is sounded: "Perhaps the friendly relations which General de Gaulle enjoyed with their leaders had something to do with it."²¹ This is said without a trace of awkwardness and with a good deal of truth. It is simply characteristic.

"Two issues fill his mind," Macmillan remarked that first summer, "Algeria and French Constitutional reform."²² Algeria was the occasion of national breakdown; it had to be eliminated. The rearrangement of what would continue to be useful and profitable imperial relationships was secondary. The principal issues were at home, in Europe, and in the wider world. On the making of the constitution

¹⁹ De Gaulle, *Mémoires*, 1: 15–16, 126–27.

²⁰ *Ibid.*, 127. But there is no evidence that he would have done differently than did the governments of the Fourth Republic in the wake of the activity of Admiral Thierry d'Argenlieu, an old Companion for whom de Gaulle continued to show deference and something like affection. See Tournoux, *Tragédie*, 48–49, 174–76. The General's subtle suggestion here is in line with, though more cautious than, the passage in his war memoirs where he declared his intention to have been to release Marshal Pétain from imprisonment on the Ile d'Yeu after two years. It might have been so, but the published avowal long after the marshal's death was unmatched by any activity on behalf of parole before the event. See Charles de Gaulle, *Mémoires de guerre* (Paris, 1954–59), 3: 250. Moreover, the president's attitude toward formal "rehabilitation" of the marshal after 1958 lent no great credence to his supposed intent in 1945. But such, perhaps, are the minor, if dubious, compensations that memorialists are tempted to extract from the bitterness of political exile.

²¹ De Gaulle, *Mémoires*, 1: 59–60, 74.

²² Macmillan, diary, June 29, 1958, in Macmillan, *Memoirs*, 4: 447.

de Gaulle has nothing new to say. The manner in which his government orchestrated the campaign for its adoption is not mentioned; nor is the way that the police were unleashed to discourage the more vociferous opponents (these pages tell nothing of the violences condoned by his regime, the assaults on the critical press, the savagery directed at demonstrators, and the atrocious and racist treatment of the wretched North Africans in France who were trapped by the dilemmas of the Algerian drama). He naturally does not recall the shabby treatment of President Coty, who was left to find his way home following the ceremony marking de Gaulle's assumption of the presidency,²³ though he provides space for Coty's encomium of his successor. In such matters reticence and understatement are not the weapons drawn from this literary arsenal. For his common sense and self-effacement, Coty would, it is true, continue to receive marks of distinction from the Elysée. For the politicians who had resisted, however, it was war then, and the campaign against them rumbles on through these volumes, a scarcely suppressed fury with those who wished no more from him than immediate salvation from the mess into which their incompetence had plunged the nation. Only "le peuple français"²⁴ had no such *arrière-pensées*. Thus from the early pages of the first volume the stage is set for the great confrontation with "the parties," as with every political assemblage, national or international, falling here under the stinging epithet of "areopagus." France had not "summoned" him to put her to sleep. His task was no less than to halt "the frightful decline which she had experienced for more than a hundred years."²⁵

Whatever the 1958 constitution provided in the way of parliament, council of ministers, and prime minister, "it was toward de Gaulle that the French turned in every case. It was from him that they expected the solution to their problems." Hence his determination to institutionalize more directly the "contract" between himself and France (he said it was on behalf of his successor, who might not have his prestige against the parties, but his general conception of the post-Gaullist era was almost Dantesque). Whatever the legal texts might say, his authority seemed *to him* not open to question. These pages reveal an astonishing self-confidence, precisely justifying the most extreme interpretations of his self-assurance, without for a moment betraying the faintest suspicion of self-interest. On the discussion of his relations with the ministers it would be hard to think of lines better communicating

²³ Tournoux, *Tragédie*, 301-02.

²⁴ Used formally in this way, the term is an idealized abstraction seeming to have little connection with "the French," of whom the General's not so private opinion was often very low. "The French," he said to Malraux, "have no more national ambition. They don't want to do anything for France. I amused them with banners." Malraux, *Les chènes*, 23. By contrast, he said that "le peuple" represented what he meant by France. *Ibid.*, 115.

²⁵ De Gaulle, *Mémoires*, 1: 40.

the master-servant relationship in government, unsullied by anything calculating or base. "All in all, I kept my distance, but not in an 'ivory tower.'" ²⁶ ("He fascinates the ministers," Antoine Pinay remarked after leaving the cabinet. "No one dares to speak."²⁷) As for parliament, what he says is of a consummate cynicism. Having almost emasculated parliamentary institutions, he confesses that he had always "revered" the oratorical talents of the tribune and that "the morose spirit of routine" into which the chambers had now fallen "filled me with melancholy," although he "consoled" himself with the reflection that the old "'games, poisons, and delights'" of the Republics had gone and taken with them all the trouble they had caused. Gaston Monnerville, *président* of the senate ("this clever Antillean"), the not so consoled and unyielding defender of the upper chamber, is written off as "dogmatic" on the subject of republican regimes. Against such dogmatics and all the jealous, self-seeking "féodalités" the presidency had to be protected by popular election. This he had known "for a long time," but he had not put it forward at Bayeux in 1946 or in 1958 because "I thought it preferable not to do everything at once."²⁸ In fact, such a proposal would have created an uproar in either year. The explanation is not flattering because false.

There follows his account of the struggle in the autumn of 1962, with every authority condemning his choice of constitutional amendment by referendum, to free the presidency from the old political notability. Here again the memorialist haughtily overrides every objection to his invocation of article 11 of the constitution (rather than article 89, which provides for amendment through parliamentary action), attacking this "armée du 'Non,'" the parties, the constitutional organs, the oldest of his political supporters, like Paul Reynaud, as then he dismissed their pleas, embassies, and public protests. Only one minister, Pierre Sudreau, failed the test and had to leave.²⁹ When the politicians carried the war to de Gaulle and censured his government he dissolved parliament. A crescendo of bitterness rises in this tale, which, even in the recollections from that remote country house seven years later, almost sears the page. If the General was so apparently calm and contemptuous of the assembly hubbub before dissolution as to attend military maneuvers, he does not entirely master his displeasure even when adversaries are dead. Hence the unjust epitaph for Reynaud, that he had "never ceased to place the

²⁶ *Ibid.*, 284, 287.

²⁷ Talk with Sulzberger, Dec. 22, 1961, *Last of the Giants*, 831; cf. the description of the Council of Ministers' meetings in Soustelle, *L'Espérance trahie*, 94.

²⁸ De Gaulle, *Mémoires*, 1: 290, 292; 2: 15-20.

²⁹ The minister of national education, Sudreau, was fearful of the precedent the president might set in so highhanded a constitutional revision. He expressed this to him in a personal interview after the event. De Gaulle said, "Oh come now, Sudreau, no one will ever have the check to do what I'm doing." Tournoux, *Tragédie*, 432.

life, the oratory and the primacy of the Palais Bourbon above everything else."³⁰ Between them, of course, lay the catastrophe of 1940, their reactions to it, and the bitter words Reynaud flung at him in 1962 from the assembly tribune. Nearly thirty years had passed since Colonel de Gaulle had waited on this man, helping him in the parliamentary campaign for military reform, seeking to serve the army, the country, and, no doubt, himself.³¹ It was Reynaud who had launched the first brief phase of de Gaulle's political career. What had happened since had cancelled out their past and whatever debt the General once had owed.

Amusingly, in light of what he writes (it is scarcely news) of ministerial dependence on him, he cites cabinet support for his stand. Hence the ministers criticized the Conseil d'Etat for its condemnation and dissolution of the Military Court of Justice (established by decree the previous June in the aftermath of the regular High Military Tribunal finding Salan guilty, but with extenuating circumstances). Obedient servants they were, but the fight against press, parties, and unions, the "Cartel," as he calls them, was carried almost single-handedly by the infuriated president. The referendum to approve the alteration of articles 6 and 7 of the constitution was successful. He writes that "a very strong majority" approved the constitutional change, but, since he gives the figures, readers with simple arithmetical skill will readily see his exaggeration. In fact they were not very brilliant referendum results, and he neglects to record that he appeared for some days to consider the withdrawal he had promised in the event of such an outcome.³² Nonetheless, the legislative elections were satisfactory. The new parliament had a strong Gaullist majority. His account of this great battle ends with another self-congratulation on having brought the people to "this marvelous transformation" of its public powers. Indeed, this was the pinnacle of his political authority. Ahead lay all the struggles for a second term in 1965, the ruinous events of May 1968, and the fatal referendum of April 1969.

Against the charge that only foreign policy and the high affairs of state much interested him, de Gaulle is quick to assert the contrary. He calculates having spent half his time on economic and social matters, giving a lengthy audit of his stewardship here. Assailing the multiple and contradictory "doctors" whose prescriptions for the national health varied so widely (Jacques Rueff was the favored physician, a poet of finance and economics), he elaborates his conception of participation. This was to be the great social nostrum of the time,

³⁰ De Gaulle, *Mémoires*, 2: 61.

³¹ A sketchy account of their cooperation in the mid-1930s and a few deferential letters to the deputy from de Gaulle are in Paul Reynaud, *Mémoires* (Paris, 1960-63), vol. 2.

³² De Gaulle, *Mémoires*, 2: 78-79, 89-90; Alexandre, *The Duel*, 95-104.

in domestic affairs, the most profound thought of the "elective monarchy." Here, too, the theme is his triumph over all the naysayers, the "féodalités" of business and labor, as he pressed forward to humanize the economic system of the modern age. "Against such swelling opposition I saw myself as the engineer, in an American film, who keeps on driving the train heedless of the alarm signal pulled by restless or ill-intentioned passengers." Undeniably, there was industrial growth, some financial stability, and agricultural change of a profound character, although he discovered daily, he says, "that economic affairs, like life itself, are a struggle in the course of which no victory is ever decisively won."³³ He does not dwell on the setback suffered at the hands of the Decazeville miners in 1962-63, whose defiance of the Elysée united clerics and Communists against him, nor on the strikes that constantly troubled the country. Among the last pages he wrote are those dealing with education, reform of the senate, and regional reorganization, cheerless issues that finally broke his hold on the nation.

In the discussion of foreign affairs the memoirs are alive with that peculiar combination of an old integral nationalist distortion of French and European history and a lofty awareness of the far regions of the globe. Here, as elsewhere, he presents himself as fated to slice through the Gordian knots. Here, too, it is the same general cast of characters: hostile governments, jealous of him, hoping if not working for his disappearance; friendly peoples, gaily manifesting their enthusiasm on his visits, and thus compelling the rulers to give heed. As in the first memoirs he is the soldier of French independence, struggling to free France from the English, the Americans, or the menacing confrontation of the superpowers. Every domestic questioning of his foreign policy is one more manifestation of "the established, promulgated doctrine" of national self-effacement. Only the people know what is good for them; or rather, only they, he, and Maurice Couve de Murville, who is pictured here as something like the sum of all virtue. He, it seems, had "le don" as few others in French diplomatic history had had.³⁴ (What that was is evident in Couve de Murville's own work, a book of record and of almost uncanny reticence.³⁵) He was the apparently perfect executant, and for

³³ De Gaulle, *Mémoires*, 1: 156, 171.

³⁴ *Ibid.*, 180-81.

³⁵ Couve de Murville, *Une politique étrangère*. Harold Nicolson, who had employed him some thirty years before as a tutor to his two sons, found that the appointment "makes me laugh," possibly because one does not expect summer French tutors to become foreign ministers, or possibly because, as he wrote to his wife, "He is so dry and plain, like a biscuit." Harold Nicolson, *Diaries and Letters* (London, 1966-68), 3: 349, 368; cf. the portraits of Couve in Paul-Henri Spaak, *Combats inachevés* (Paris, 1969), 2: 365-66; and Soustelle, *L'Espérance trahie*, 97-98.

his discretion, fidelity, and relentless hard work he is singled out in these pages.³⁶

In the liberation of Europe and the establishment of French primacy there, Germany was the key. De Gaulle recollects the manipulation of Konrad Adenauer without excessive modesty and imparts that the chancellor's distinguished record did not spare him, on their first meeting at Colombey, a blunt if not wholly accurate rehearsal of three wars launched against France since 1870. (Adenauer's lengthy account makes no mention of this opening gambit; nevertheless, each appears to have delivered an expansive discourse on the past that was doubtless more spontaneous and sparkling than the remembered version set down.)³⁷ The tone is one of considerable satisfaction ("I invited Adenauer whom I scarcely knew," he remarked to Malraux, "you get people who despise each other, because they don't know each other, to sit down to the same leg of lamb, and that transforms them into sheep"³⁸). He naturally does not mention Adenauer's anger with him for the attempt, immediately following this September 1958 house party (it was exactly two days later), not so much to reconstruct Europe as to set up a three-power Atlantic directorate.³⁹ The memorialist's purpose, rather, is to lead up to the historic January 1963 treaty of friendship and cooperation, and, despite occasional difficulties with Bonn, the impression given is of great harmony. The troubled days lay ahead, after Adenauer had gone (when, for instance, de Gaulle would hold forth to Harold Wilson on the theme of a prospective new threat, since "Germans will always be Germans"⁴⁰).

In some contrast is his discussion of the British, whose principal object in those early days was the destruction of the European Economic Community. As he remembers it, Prime Minister Macmillan immediately warned him that the Common Market would be "the Continental Blockade" in a new war, economic at first, but threatening to spread to "other fields."⁴¹ Everything here builds toward the famous veto of January 1963, for by that time, of course, the issue was not dismantling

³⁶ Of the private critiques later reported ("this wrinkled unctuous fellow") there is naturally no suggestion here; see Frédéric Barreyre, *Les derniers mots du Général* (Paris, 1971), 70.

³⁷ Konrad Adenauer, *Erinnerungen* (Stuttgart, 1965-68), 3: 424-35.

³⁸ Malraux, *Les chénes*, 173; cf. Couve de Murville, *Une politique étrangère*, 37, 241. It is not absolutely certain that Adenauer was wholly delighted with the visit. Terence Prittie discovered German and British officials who believed that de Gaulle had quickened serious reservations in the chancellor at that time. Terence Prittie, *Konrad Adenauer 1876-1967* (London, 1972), 264-65.

³⁹ De Gaulle appears to have communicated a copy of his September 17, 1958, memorandum to Paul-Henri Spaak, secretary-general of NATO. Spaak then permitted the German and Italian ambassadors in Paris to see it. Macmillan, *Memoirs*, 4: 453-54; cf. Spaak, *Combats*, 2: 180-81, where he says that this copy later disappeared from his files. The episode is not, to say the least, too clear. Curiously, Adenauer himself does not refer to it in his recollections.

⁴⁰ Harold Wilson, *The Labour Government 1964-1970: A Personal Record* (London, 1971), 412.

⁴¹ De Gaulle, *Mémoires*, 1: 199. It is of some interest that Macmillan's diary entry for this day in question, June 29, 1958, at least as published, makes no mention of any such dire warning.

the EEC but British entry into it. Patiently the General exposes the unreadiness of his neighbors to shed their dependency on America and to turn toward Europe. Nevertheless, he offers sympathetic accounts of the visits back and forth across the Channel, not least of the honors done him during the state visit of 1960, in particular the great ceremony in Westminster Hall. But he was above influence, public or private. In November 1961, while entertaining de Gaulle at his Sussex house, Macmillan noted:

Charming, affable, mellow as the General now is, his little pin head is still as small as ever. His views are inward, not outward looking. I fear he has decided to oppose us, yet, in a way, he wants us in Europe. . . . Sometimes, when I am with him, I feel I have overcome it. But he goes back to his distrust and dislike, like a dog to his vomit.⁴²

De Gaulle's thoughts on these occasions seem likely never to be known for certain. But his analysis of English weakness is pitiless, and his conviction that the British were preparing to wreck the EEC from the inside was evidently unshakable. He foresaw the day when he would have either to veto the Brussels negotiations or to end French participation in the Common Market, since no one else in the Six would say "No!" to England.⁴³

He throws no light on his September 1958 approach to Great Britain and the United States for a three-power Atlantic directorate; he confirms that he expected evasive replies and thus freedom to begin the withdrawal from all military cooperation with NATO.⁴⁴ Almost overnight he had ended the long French subordination to the Anglo-Saxon leadership, and although there are only the friendliest personal references to President Eisenhower here, the note of satisfaction with his own achievement is as clear as it is justified. With the Russians also, he maintains, his policy was both firm and independent, and he established with Chairman Khrushchev "a real man to man contact."⁴⁵ Certainly the Russian leaders' state visit to France in March 1960 provides one of the few mildly amusing moments in these largely high-minded reminiscences, all too regrettably stripped of their author's lethal wit. The particular moment came at Rambouillet, with Kosygin rowing de Gaulle and Khrushchev around a pond to the accompaniment of the General's slightly sarcastic badinage and the chairman's coarse

⁴² Macmillan, diary, Nov. 29, 1961, in Macmillan, *Memoirs*, 5: 426, 428.

⁴³ De Gaulle, *Mémoires*, 1: 200.

⁴⁴ *Ibid.*, 1: 214-15, where he misdates the memorandum September 14; Couve de Murville, *Une politique étrangère*, 33-34, is even more laconic and dismissive of the approach. "The 1958 memorandum was only a diplomatic pressure tactic," de Gaulle is said to have remarked later on. "I was then looking for a way to get out of the Atlantic alliance and to regain the freedom of action that had been surrendered by the Fourth Republic. . . . Hence I asked for the moon. I was sure they would not give it to me." Quoted in Tournoux, *Tragédie*, 321.

⁴⁵ De Gaulle, *Mémoires*, 1: 238.

humor.⁴⁶ But beyond these familiar tales of three men in a boat and the bold defiance of the American hegemony in Europe he has little to tell.

At the abortive summit conference of May 1960 he is naturally the central figure, if not the hero, evidently suffering no nonsense from Khrushchev while counseling the shaken Eisenhower (who later recalled de Gaulle touching his elbow and saying, "Whatever happens *we are with you*"⁴⁷). From the outset he had taken a strong line over Berlin, and here again he accuses both of his Western partners (at their earlier December 1959 Paris conference) of showing weakness, especially Macmillan, who is said to have been upset by the prospect of nuclear war for the sake of a German city. Thus he had given them a lecture on standing up to the Russians. How much more impressive it may be in these pages than it was in reality one can only guess, but there is certainly no English equivalent for the magnificent hauteur with which he recalls a sharp encounter with the rough-and-ready chairman: "M'enveloppant de glace, je fais comprendre à Kroutchev que la menace qu'il agite ne m'impressionne pas beaucoup."⁴⁸ It is one of the many miseries of the English language that this kind of thing cannot be said, even if one wished to say it. Hence Anglo-Saxon skepticism that it is worth saying in any language, and hence Macmillan's conclusion that de Gaulle's intransigence was a mere device to impress Adenauer ("who usually sucks up to de Gaulle"⁴⁹), while counting on the British and the Americans to get him off the hook: "If de Gaulle thought there was any real danger of war, he would be in a panic."⁵⁰ It may well have been so, for, asked some years later by Harold Wilson whether he cheated at patience, the General replied that there were situations when this was justified.⁵¹

Confirmation of such suspicions will not be found here. The memoirs are full of set pieces and formal portraits, with few unaware touches. If there is an almost excessive elevation of tone (broken only by the occasional mordant thrust), they are not wooden, like the Eisenhower memoirs; they are not whimsical and chatty, like the lengthening Macmillan diaries and commentaries; they are undeniably his own,

⁴⁶ The sole comment on de Gaulle in *Khrushchev Remembers* (Boston, 1970), 507, is "and he's a sober-minded man."

⁴⁷ Dwight D. Eisenhower, *The White House Years* (New York, 1964-65), 2: 556.

⁴⁸ De Gaulle, *Mémoires*, 1: 241.

⁴⁹ Macmillan, diary, Oct. 22, 1959, in Macmillan, *Memoirs*, 5: 93.

⁵⁰ "He really admitted this to me. He said it was not perhaps anything but a rather cynical policy. Yet it was justified for we must at all costs prevent another German 'myth', such as had made Hitler's rise to power possible." Then de Gaulle went on to say that he would take this line even if Adenauer should accept the British view. Macmillan asked, "You would be plus royaliste que le roi?" De Gaulle replied, "Certainly. Then the Germany of the future will know that France was true." Macmillan, diary, Nov. 29, 1961, in Macmillan, *Memoirs*, 5: 426.

⁵¹ Wilson, *Labour Government*, 406.

as the Khrushchev recollections may or may not be. But they present the General in a consciously imperial pose, gravely, if sometimes mechanically, commenting on the state of India or the city of Toronto ("industry was very active, construction was thriving, the university flourished"), discussing with Richard Nixon the difficulties of getting the press to take one at one's word, contemplating the vast hopes placed in President Kennedy until, "having taken each other's measure, we continued on our way, each of us bearing his burden and moving on toward his destiny!"⁵² Clearly de Gaulle in retirement wanted to communicate only the historic personage, "the myth," as he used to say, that posterity should know, even to his work habits and his relationships with his family, but tidily and in a rigidly controlled manner. ("History," he said to Malraux, "may justify life, it does not resemble it.") And one can only admire the methodical manner in which this elderly man measured out his pages, assigned his paragraphs, assembled his brief character sketches, and rounded out his chosen subjects. As memoirs these chapters are both disciplined and impassioned with a cold and distant fury. On the one hand there is a compulsive underlining of uncertain triumphs, some of which were no more than the ephemeral brilliance of a particular *conjoncture*; and on the other, a bleak chronicling of a life without illusions: "I'm the character in Hemingway's *Old Man and the Sea*: I've brought back only a skeleton."⁵³ As is so often remarked, the key to this historic personage was written long ago, in the collection of essays titled *The Edge of the Sword*, with their definition of "the man of character" and their reflections on the nature of "prestige."⁵⁴ These last pages, though suddenly stopped short, continue the depiction of the exceptional leader adumbrated forty years before.

What is missing, of course, is Charles de Gaulle. "Now that he is old [sixty-nine] and mellowed," Macmillan observed in March 1960, "his charm is great. He speaks beautiful, rather old-fashioned French. He seems quite impersonal and disinterested." The memoirs consciously hide the man. "What did Caesar *believe*?" he said to Malraux. "Nothing he wrote tells us."⁵⁵

⁵² De Gaulle, *Mémoires*, 1: 255, 271.

⁵³ Malraux, *Les chènes*, 71, 79.

⁵⁴ *Le Fil de l'épée* (Paris, 1932), in a good translation, *The Edge of the Sword* (New York, 1960).

⁵⁵ Macmillan, diary, Mar. 13, 1960, in Macmillan, *Memoirs*, 5: 183; Malraux, *Les chènes*, 73. Doubtless many of those who knew him well will reveal him little by little, but the portrait will require a thousand tiny details if it is convincingly to replace the formal mask that the General left behind. For example, the solemnity of the Washington funeral in 1963 was quickly followed by a bantering exchange with John Kenneth Galbraith, who remarked that tall men, being more visible, were more to be trusted. De Gaulle shot back, "It is important that we be merciless with those who are too small." J. K. Galbraith, *Ambassador's Journal: A Personal Account of the Kennedy Years* (Boston, 1969), 598.

He said that writing was torture, that it was slow, that it was useless, but each time he returned to it.⁵⁶ Whether this was because he did, after all, think there might be some relationship between the idealized “peuple” à la Michelet with whom he lived out his “contract” with France, and the “cattle” and worse, as he branded the French in his fits of anger; or because he felt compelled to write his epic as the necessary extension of the action twice broken off and denied him; or because he was a man of letters with an esthetic need to complete his work of art (as once before, with the first memoirs, he had so evidently done⁵⁷), it would be difficult to say. And his question to Malraux remains without answer.

⁵⁶ “He smiled quite sadly and said: ‘Retire? Yes. Write another volume? No. Writing about the war; that was one thing. Now, that is another. Retire? Yes. But not another volume. Colombey? Yes. But not writing.’” Interview, Jan. 20, 1962, in Sulzberger, *Last of the Giants*, 843; on writing, see Tournoux, *Tragédie*, 233, and Malraux, *Les chênes*, 34; on the marketing of, and the writing of dedications in the memoirs, see Chapus, *Mourir à Colombey*, 223–31.

⁵⁷ De Gaulle, *Mémoires de guerre*, 3: 287–90.

Reviews of Books

GENERAL

MAURICE LOMBARD. *Monnaie et histoire d'Alexandre à Mahomet*. (École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences économiques et sociales. Centre de Recherches Historiques. Civilisations et sociétés 26. Études d'économie médiévale 1.) Paris: Mouton. 1971. Pp. 233. 36 fr.

This is a strange book. Undocumented theses and annoying inaccuracies make it a worrisome volume. Yet the late Maurice Lombard possessed a brilliant mind, and so this work, rich in insights, expectedly furnishes pleasurable and instructive reading for the economic historian. On balance, then, the piety of Lombard's disciples in seeing his research into print deserves praise; I urge them to keep their promise to publish one or two more of his studies.

The premise of this volume is that there was an ancient and a medieval precedent for such momentous economic events as the flood of American bullion into sixteenth-century Europe. Lombard contends that the conquests of Philip and Alexander created a Eurasian monetary ecumene based on gold, stretching from West Africa to India. Economic resources hitherto locked within separate and hostile orbits, he would argue, were then released into the brilliance and openness of Hellenistic civilization. Lombard unfortunately provides few citations to support his views; moreover, the reader is left on his own to ponder multiple chicken-egg problems that arise in the dust of the author's fast tour of ancient economy.

Lombard then leaps to his medieval example. The Muslim conquests, he declares, resulted in a similar monetary world system based on gold, one that pumped life into a Western and an Eastern Europe creaking along on insufficient gold supplies. The argumentation for this pri-

macy of gold in the Islamic Empire is downright sloppy and some ten years out of date. Lombard all but ignores, for instance, the huge finds of Muslim silver coins in the Baltic region.

The remainder of the book contains valuable discussion of some other matters relating to Muslim economic activity from the eighth to the eleventh centuries. For example, Lombard's examination of Europe's exports to Islamic areas—slaves, furs, wood, and metals, including swords—should, if correct, reopen old debates concerning the influence of commerce in human bondage and war materials in the development of Western Europe's economy. Equally valuable is his treatment of the growth of Muslim towns—one of the great urban moments in history, Lombard states, in comparing it to the Hellenistic period. In addition, the author presents an informative description of the social repercussions deriving from the ascendancy of merchants in these Islamic cities. Landed families thereby declined in importance, he says, and a rural and urban *lumpenproletariat* suffered considerable misery. Noteworthy also are Lombard's sensible remarks concerning the early activity of Muslim guilds, a subject most Islamicists are foolishly avoiding.

The bibliography of sources in the first third of the volume will be useful to economic historians. This catalog lists Eastern and Western texts in which Lombard found materials relevant to the Muslim economy from the eighth to the eleventh centuries. Lombard's book has glaring faults, but his vision peered beyond stereotypes. The insights and the bibliography of sources make the volume worthy of its price.

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MIKLÓS JANKOVICH. *They Rode into Europe: The Fruitful Exchange in the Arts of Horsemanship between East and West*. Translated by ANTHONY DENT. New York: Charles Scribner's Sons. [1973]. Pp. 175. \$12.95.

They Rode into Europe will be of particular interest to readers concerned with such varied fields as the history of transportation, the dissemination of technology, sociological developments, and the evolution of military practices and institutions. This work is valuable because the author has written from the East European point of view, from the geographical region where Asian and European struggled for supremacy over more than a millennium. More important is the fact that Jankovich has been able to utilize sources that are usually unavailable to the Western scholar, unless he is able to use the Magyar and Slavic languages.

The scope of the work is vast. It begins with an account of the domestication of the wild horse and of attempts, often successful, to break onagers, camels, and even reindeer to harness and the saddle. The history of what the author not inappropriately calls "the age of the horse" is continued until it was supplanted by the steam locomotive and the internal combustion engine. This is done in 154 pages of text, plus an additional 12 pages of notes, and even the most casual reader will lament that the author has not gone into more detail on this or that aspect of horsemanship. These deficiencies are offset, to some extent, by illustrations on a lavish scale, in color and in black and white, and by numerous line drawings in the text.

The serious scholar, however, will find that *They Rode into Europe* is disappointing in a number of respects. The documentation is inadequate and many a conclusion is reached with no indication of the grounds on which it is based. The bibliography is notably lacking in Western sources; for example, Widukind and Luitprand of Cremona, authorities on the irruption of the Magyars into Germany and Italy, are omitted. Even more serious is the failure of Jankovich to consider the influence of Moorish horsemanship in Spain upon the cavalry tactics in early modern Europe. Lynn White's significant work on cross-cultural influences in a civilization based on horsepower is ignored, as well as the more recent studies of B. S. Bachrach. Finally, it should be noted

that this is a translation of a German version of a Hungarian original. The publishers nowhere state the qualifications of the author to write such a book, the faithfulness of the German translation to the Magyar text, or the grounds on which the translator was selected. By Western standards, this is not a scholarly publication.

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HANS HUTH. *Lacquer of the West: The History of a Craft and an Industry, 1550-1950*. [Chicago:] University of Chicago Press. 1971. Pp. x, 158, 364 plates. \$25.00.

In this much expanded version of his popular *Europäische Lackarbeiten* Hans Huth has provided a substantial picture of the European lacquer craftsman's work from the mid-sixteenth to the mid-twentieth centuries. Handsomely illustrated, both in black and white and in full color, the new book incorporates Huth's subsequent research findings, until now only available in scattered periodical sources. The study is concerned with Western objects "made of wood, papier-mâché, tin, leather and earthenware . . . decorated to produce the highly polished effect of lacquer."

The first wave of lacquer work reached Europe by way of the Middle East between 1550 and 1630. The technique was mastered by Venetian craftsmen and found its use in the decoration of boxes, chests, cabinets, musical instruments, and mirror and picture frames. The products of this period reflect Islamic sources, particularly Turkish bookbindings and weapon ornamentation, and show no indebtedness to the Far East.

Far Eastern lacquered objects began to appear in Europe toward the end of the sixteenth century but not in sufficient quantity to satisfy the demand. The idea of creating a reasonable Western substitute seems natural although there is scanty documentation, Huth believes, to suggest European production between 1630 and 1670. By 1680, however, "European-produced lacquer rivalled that imported from the Far East."

The basis for this new craft (and ultimately for a substantial industry) was laid in 1720

when Father Filippo Bonanni published the first "accurate and authoritative account" of Chinese lacquer, identifying its chemical base in Oriental resin. The impossibility of obtaining this in the West led Bonanni to suggest the use of substitute techniques based on varnish or European shellac. These adaptations were variously referred to as Japan work, *vernīs de la chiné*, or *Indianish werk*. Oriental products and European imitations both were popularly called lacquers.

The second phase of lacquer work in Europe coincided with the eighteenth century and its interest in *chinoiseries*, representations of the Orient and things Oriental by Western artists and decorators. At first these representations tended to dignify and exalt the Orient. Later a withdrawal of Church sanctions led to an emphasis on the droll, the grotesque, and the comic aspects of the exotic. European flowers and *rocaille* displaced the vogue for *chinoiseries* at mid-century. With the late-century romantic revivals a new treatment of China as a fairy-land of pleasant landscapes peopled with sentimental figures came into style, following the manner of Boucher and Pillement. In the late eighteenth century lacquered objects lost most of their foreign characteristics and began to follow closely upon European fashion shifts.

Chinoiseries were produced in every major country either by professional craftsmen and decorators or by aspiring dilettanti. Among those of the eighteenth century who we know by name are Gérard Dagley of Berlin, Martin Schnell of Dresden, Giles Grendey of London, members of the Martin family of Paris, and Thomas Johnson of Boston.

When it became economically unfeasible for self-employed lacquerers to continue their trade after the French Revolution two new factors aided the craft in becoming a popular industry in the nineteenth century: the production of tinware and papier-mâché. Huth traces the rise of the japanned tinware industry in Pontypool, Usk, Birmingham, and Bilston and the papier-mâché industry in Wolverhampton, Birmingham, and Scotland. Of unusual interest is Huth's research on Stobwasser of Brunswick and on the Russian enterprises of Demidoff, Korobov, Lutkins, and others. To some extent this material parallels Walter Holzhausen's 1959 study, *Lackkunst in Europa*. Both authors

give a nod to lacquerers practicing in the twentieth century.

Plates 148, 150, and 151 illustrate a "Clavichord, by H. A. Hass, Hamburg, 1732." The instrument is a harpsichord, however, and should be so labeled in the second edition of this work—otherwise so free of errors. One also hopes that Huth will expand, in the future, his evaluations of the 1630–80 evidence as well as his discussion and illustration of late eighteenth- and early nineteenth-century lacquered furniture, the latter a topic more fully managed by Hugh Honour in his 1961 volume, *Chinoiserie, The Vision of Cathay*.

Hans Huth's *Lacquer of the West* is the stunning contribution of a mature connoisseur and art historian who has never for a moment ceased to be in love with either the subjects or the objects he describes. The book is therefore an enviable model of humanistic scholarship.

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ANTHONY D. SMITH. *Theories of Nationalism*. (Torchbook Library Edition.) New York: Harper and Row. 1971. Pp. viii, 344. Cloth \$12.00, paper \$3.95.

BOYD C. SHAFER. *Faces of Nationalism: New Realities and Old Myths*. New York: Harcourt Brace Jovanovich. 1972. Pp. xv, 535. \$12.95.

These two books offer a comparison between the approaches of two different disciplines, sociology and history, to the study of a phenomenon of interest to both: nationalism. Both books reveal some of the strengths and weaknesses common in the practice of each discipline. The study by sociologist Anthony D. Smith examines the various theoretical models that have been advanced for the analysis of modern nationalism, indicates the problems of each, and offers a model of its own; Smith's hypothesis is analytical, highly abstract, and largely devoid of the kind of evidence historians demand in support of generalizations. The work of historian Boyd C. Shafer, on the other hand, traces the evolution of national sentiment from the eighteenth century to the present and offers some conclusions about nationalism in the present and future; the book is descriptive, filled with historical examples and quotations illustrating the different "faces" of

nationalist doctrine, but it lacks any genuine conceptual framework that would attempt to analyze the social forces that generate nationalism. Smith begins with the questionable assumption that nationalism must be studied as an ideological movement, that "the growth of nations and the rise of nationalism can be separated" (p. 6). Shafer begins with the equally questionable opposite assumption that "the nation makes patriots," that all the activities of national governments, from the centralizing bureaucracy of Louis XIV to the New Deal of Franklin Roosevelt, are responsible for the growth of nationalism. Attempting to avoid all value judgments, Smith adopts an attitude of "ethical ambivalence"; Shafer, while acknowledging his bias against extreme nationalism, readily offers his own values when he concludes that the nation has protected man against "the Hobbesian anarchy that ever lurks behind the veneer of human politics," that it has given man "the feeling that he is not alone, that he belonged to a group that cared" (p. 350). The historian interested in more discourse between history and sociology will be discouraged to discover how little these scholars seem to have learned from each other's disciplines.

Smith objects to the "idealist" analysis of Kedourie, which sees modern secular thought as the primary solvent of traditional social institutions and thus Kant as the fountainhead of modern nationalism. He finds that most of the views of nationalism as a response to the crisis of modernization (as espoused in various ways by Eisenstadt, Smelser, Lerner, and others) are tied too closely to the Western experience and thus are ethnocentric and deterministic. He disagrees too with the more complex treatment of the relationship between nationalism and modernization offered by Gellner because he believes that it exaggerates the link between nationalism and the aspirations of an uprooted proletariat, that it overstresses both the unanimity of the intelligentsia behind nationalism and the linguistic criterion for nationhood. Nevertheless, Smith's own analysis draws much from these other theories. He begins by drawing a distinction between what he calls "ethnocentric" and "polycentric" nationalism. The former is a movement against foreign rule; it regards outsiders as barbarians and seeks only to preserve its own culture; the latter sees the

world divided into separate nations, each with its own value and autonomy. The question he addresses is: how does the transition from "ethnocentric" to "polycentric" nationalism come about? The answer, he suggests, is to be found in the conflicting responses of the intelligentsia to the tensions produced by modernization. Modernization produces an essential conflict between the religious and cultural values of the traditional order and the rational and efficient claims of the "scientific state." Both systems exert legitimate claims for authority and the intelligentsia responds to this "dual legitimation" in three ways: the "traditionalist" rejects modernization entirely, the "assimilationist" embraces it completely and sets his hope on an enlightened cosmopolitanism, and the "reformist" tries to combine both traditional and modern society into one synthesis. Neither the reformist nor the assimilationist can realize his objectives completely, however, and the result is that the former tries to save the unique values of his culture, which leads him toward an ethnic definition of community, while the latter abandons his devotion to common humanity and substitutes a belief in the community of nations—"polycentric nationalism."

Shafer's argument is much less complicated. He summarizes it best himself: "This book has argued that the nation and nationalism developed because the governments (early the dynastic, later the democratic or the authoritarian) increasingly penetrated the lives of people, because people increasingly participated in the national affairs and identified themselves and their political and cultural interests with those of their respective nations, that out of the penetration, participation, and identification there came to be a kind of consensus within each nation, that once this was established nationalism fed upon itself and grew, and that the national states, once established, encouraged and imposed nationalism, educating and persuading the already loyal to be more patriotic by many means and coercing the reluctant by law and by force" (p. 345).

Shafer's volume contains a great deal of information, useful notes, and an extensive bibliography; his overall approach, however, offers little that is new, and it uncovers no promising new trails for future exploration.

Smith's approach is admittedly too schematic and too tidy; it lays too much stress on the role of the intelligentsia. Despite its obvious faults, however, the book does suggest some new paths historians should follow if we hope to learn more about the complex topography of nationalism.

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T. BENTLEY DUNCAN. *Atlantic Islands: Madeira, the Azores, and the Cape Verdes in Seventeenth-Century Commerce and Navigation*. (Studies in the History of Discoveries: The monograph series of the Society for the History of Discoveries.) Chicago: University of Chicago Press. 1972. Pp. xv, 291. \$10.00.

Islands possess a fascination *sui generis*. Centers of communication, they have a strategic, commercial, and navigational importance out of proportion to their size. The Azores, Madeiras, and Cape Verdes comprise twenty-nine islands. Each has its own personality, ethnic composition, climate, and topography and different degrees of habitation, cultivation, and prosperity. From the sixteenth century there was considerable interinsular trade in complementary raw materials. But the historical importance of these archipelagoes lay in their geographical position as pivots of international trade. The axes of colonial Atlantic commerce—London-Boston, Seville-Caribbean-Central America, Lisbon-West Africa-Brazil, West Africa-Caribbean-North America—took advantage of the islands as sources of manpower and supplies, havens for storm-tossed ships, refuges for smugglers, and commercial entrepôts for the supply or transshipment of raw materials and manufactured goods. Funchal and Horta in the seventeenth century boasted an international mercantile elite and a cosmopolitan outlook. The Azores were vital to the slave trade between West Africa and the Americas. Dr. Duncan demonstrates how outmoded the geometrical clichés of triangles and polygons are in describing the cat's cradle of Atlantic routes bearing greater or lesser densities of traffic.

The prosperity of the islands did not lie in the commercial acumen or initiative of their peoples. The destiny of the Atlantic islands was decided in Europe, the Americas, and, to a lesser degree, Africa. Reduced to a passive role, the

islands were the victims of commercial and political vicissitudes. Whereas English sallies into the Guianas and Lesser Antilles and the seizure of Jamaica opened up new markets for Madeira and the Azores, the import of cheap indigo from Spanish America dealt a mortal blow to Azorean pastel production, and the rise of a whisky industry in the United States deprived Madeira of a major market. The ebb and flow of economic fortune was no better illustrated than by Madeiran sugar production in the sixteenth and seventeenth centuries, until Brazil came to dominate the trade following the expulsion of the Dutch in 1654. Dr. Duncan has ably delineated the flux and reflux of prosperity and decline in the basic commodities of each island.

The author has performed a valuable service in emphasizing the vital role of the islands in the building of the Portuguese, English, and Spanish seaborne empires. Archives in Portugal, the Azores, and Madeira have been consulted; it is regrettable that Dr. Duncan was unable to consult Brazilian, Spanish, English, and North American manuscript sources. Portuguese colonial accounting methods are the despair of any modern scholar, but the presentation of the available figures on imports and exports from the islands is markedly unsophisticated and lacks analytical maturity. The book would have benefited from more careful editing: the author's descriptions based on personal experience often disrupt the historical narrative; statistics of shipping for the years 1875-98 and of coal and oil sales at Mindelo from 1927 to 1932 and from 1952 to 1957 have no relevance to the central theme. The cartography is of a high standard, and the descriptive map of trade between the Cape Verdes and Guinea is a model of its kind. *Atlantic Islands* whets the reader's appetite but will not stir his imagination. Asking more questions than it answers, this monograph will, it is hoped, stimulate further research into the role of these islands in the formation of the Atlantic community.

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L. A. SHUR. *K beregam Novogo Sveta: Iz neopublikovannykh zapisok russkikh putesthestvennikov nachala XIX veka* [To the Shores of the New World: From Unpublished Reports of

Russian Travelers at the Beginning of the 19th Century]. (Akademiia Nauk SSSR, Institut Etnografii imeni N. N. Miklukho-Maklaia.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 285.

More than two centuries ago the imperial Russian double-headed eagle swooped down over North America and grasped Alaska in her talons. Alaska, the land of fur gold and Stone Age Aleuts, Eskimos, and Indians, fell prey to Russian fur hunters, merchants, and Cossack banditti who exploited the fur wealth and enslaved the nearly defenseless natives. In the early 1800s the Russian-American Company, a Russian fur trade monopoly in Alaska, occupied Spanish-claimed northern California at Fort Ross and Bodega Bay.

In *K beregam Novogo Sveta* Leonid A. Shur, a noted Soviet historian of Latin America, published for the first time three Russian travel diaries describing the history and culture of early nineteenth-century Brazil, Peru, Mexico, California, and Alaska. Fedor F. Matiushkin and Fedor P. Litke, young Russian naval officers, kept the first two diaries while traveling around the world from 1817 to 1819 on the sloop *Kamchatka*. Their comments on the Russian settlements at Fort Ross and Bodega Bay are particularly valuable, since before 1820 few firsthand descriptions of these outposts exist.

Baron Ferdinand P. Vranghel, famous arctic explorer and general manager of the Russian American colonies, wrote the third diary while journeying from Sitka to St. Petersburg through Mexico in 1835 and 1836. His diary reveals his unsuccessful attempts to convince Mexico to establish trade relations with Russian America. Unfortunately, Shur failed to note or make use of Vranghel's diary of his Mexican trip which is found in the United States National Archives' *Records of the Russian-American Company, 1802-1867: Correspondence of the Governors General, Communications Received* (volume 10, folios 189-98). This omission is inexcusable, especially since microfilm copies of these documents are in the Soviet Union.

Well illustrated with rare nineteenth-century drawings, Shur's publication is a priceless contribution to a thorough understanding of Russian activities in the New World.

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DAVID W. COHEN and JACK P. GREENE, editors. *Neither Slave nor Free: The Freedman of African Descent in the Slave Societies of the New World*. (The Johns Hopkins Symposia in Comparative History, number 3.) Baltimore: Johns Hopkins University Press. 1972. Pp. xi, 344. \$13.50.

This book is a distinctive contribution to the enticing but treacherous domain of comparative history. It succeeds because it is written by qualified scholars who address a delimited, manageable subject—the role of the free colored population in New World slave societies. Six of the papers were given at a conference in 1970 and then revised. Three more were written, and one reprinted from elsewhere, to fill gaps noted in the discussions. This procedure helped to eliminate the randomness and incommensurability that plague many such collections. The task was to canvass current knowledge and pinpoint areas of needed research regarding two topics: first, the experience of the free colored as a measure of the character of slavery and race relations; second, the functional roles of this group in the evolution of the respective societies.

Eschewing the usual piety and rhetoric of anthologists, the editors have synthesized their contributors' comparative data to highlight contrasts and general trends. Tables display the percentage of freedmen in each society with respect to total, free, and colored populations for the period from 1764 to 1840, demonstrating the low proportion for the United States South and the British West Indies as against Ibero-America. In every society, manumission of white males' black and mulatto wives or concubines and their offspring yielded large increments to the free colored group, as did self-purchase, especially by males, in times of economic growth. Furthermore, each society experienced periods of restriction on manumission, when natural replacement became the main source of free-colored growth. The editors tentatively conclude that freedmen tended to acquiesce in the slave system and to avoid forming a common front with slaves, although frequently they raised political issues damaging to the slavery.

Availability of basic research importantly determines space allocation and strategies of presentation. In a mere twenty pages Eugene

Genovese sketches a trajectory with local variations for the United States South, while Jerome Handler and Arnold Sio take twice as long to construct a story, largely from sources, for tiny Barbados. (Total population and territorial size, however, can be misleading indicators; readers may be surprised to learn from Franklin Knight that from the late eighteenth century to 1860 the free colored population of Cuba was numerically about equal to that of the United States South.) In a smoothly written essay Frederick Bowser generalizes as best he can for three centuries of regionally diverse Spanish American colonial history, frequently drawing on his own Peruvian research for illustration. John Russell-Wood tackles the similarly complex assignment of colonial Brazil by focusing on three major nuclei of colored population: the Bahian sugar zone, the mining zone, and the city of Salvador.

Some of the book's most arresting hypotheses arise from single papers rather than from the ensemble. A notable instance is Hermannus Hoetink's essay on the Netherlands Caribbean, which shows that a free-colored elite developed in Surinam, with its severe slave system, but not in Curaçao, with its mild, paternalistic system. Other articles treat the French Antilles (Léo Elisabeth), Saint Domingue (Gwendolyn Hall), Jamaica (Douglas Hall), and nineteenth-century Brazil (Herbert Klein). One hopes for a sequel by equally knowledgeable scholars that would consider the adjustment of freedmen in post-emancipation societies.

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PETER CALVOCORESSI and GUY WINT. *Total War: The Story of World War II*. New York: Pantheon Books. 1972. Pp. xiii, 959. \$15.00.

Twenty-eight years after the most destructive war in human history the memories of those who fought in it have begun to fade; new generations have grown up for whom the war is but a blurred panorama of distant battles and unfamiliar names. To bring the causes and events of the war back into focus the authors decided "to write a book showing why it happened and how it went" (p. xi). In spite of the considerable difficulties involved in such an undertaking Messrs. Calvocoressi and Wint have succeeded in presenting a lucid and well-written

account of the diplomatic, political, social, economic, and military aspects of the war. They have also managed to include enough background material to make even the most complicated developments comprehensible to the average reader.

To cope with the enormous amount of material the authors have divided their story into two parts. Mr. Calvocoressi writes on the war in Europe, on the Atlantic, in Africa, and in the Middle East, while Mr. Wint, and after his death, his wife, covers the war in Asia and the Pacific. It is a sensible arrangement and the minor overlapping that occurs does not distract from the even flow of the narrative. The major problems of writing a one-volume history of a period as complicated and confusing as the Second World War are selection and organization of the material; the authors should be commended on both counts.

The origins of the war are a confused and tangled web, and Calvocoressi stresses correctly that there were several wars that only merged into one global conflict after Pearl Harbor. Hitler's policies played a crucial role in unleashing the war in Europe, and in explaining Hitler's policies Calvocoressi presents a good summary of modern German history and the rise of nazism. Inevitably there are some misconceptions: Bismarck did not use racial prejudice for political purposes, and Hitler's plans for Germany's expansion included considerably larger territories than those lost after World War I. For the rest, Calvocoressi's account of developments between the wars is balanced, concise, and eminently sound. In his view, "Hitler's political gamble came off not because France and Great Britain were militarily incapable but because they were strategically inept" (pp. 94-95).

The initial stages of the war—the Polish and Norwegian campaigns and the Russo-Finnish war—are well summarized, but Hitler's contribution to Manstein's plan for the invasion of France is not adequately discussed; on the other hand, the Führer's ambivalent attitude toward Britain and the German army's unexpected halt before Dunkirk are clearly explained. The Battle of Britain, the campaigns in the Balkans and North Africa, and Hitler's preparation of "Barbarossa," the invasion of Russia, are well described. Calvocoressi does not

believe that Germany's Balkan campaign had any appreciable effect on "Barbarossa" (only if the British defense of Crete had lasted longer would it have made any difference), but he fails to explain Stalin's persistent refusal to heed numerous warnings of the invasion.

Germany's New Order, territorial adjustments, population movements, and the extermination of Jews and other minorities are well covered, as is the relationship between Britain and the various European governments in exile. Of the neutrals, only Switzerland's role in the war is mentioned; Sweden, Spain, and Portugal are omitted. The European resistance movements, including the German opposition to Hitler and the revolutionary movements in southeastern Europe, are carefully explained. The United States-British-Russian alliance and the issue of the second front are put into proper perspective. One of the best chapters deals with British domestic problems during the war. That the British kept on fighting in spite of blatant inequalities at home and seemingly insurmountable odds abroad has puzzled many observers. "The British did not rebel against inequality and injustice," Calvo-coressi writes, "because on the whole they did not believe in equality or expect justice. . . . Freedom and incorruption were the secular religion of Great Britain . . . and *in hoc signo* the British engaged the enemy as a united people" (p. 406).

In judging the Allied bombing campaign and its effects on Germany's war economy Calvo-coressi concludes that Germany's Achilles' heel was her fuel oil production and her transportation system, while her greatest economic weakness was the inefficiency of her administrative machinery. But bombing did not win the war. By the time it became fully effective—in the fall of 1943—the Russians had already inflicted heavy defeats on the German army.

The section on the war in the Far East and the Pacific is shorter, though equally well written and organized. There are excellent background chapters on China, India, and Japan, and the major military and naval campaigns are adequately dealt with and lucidly described. There are concluding chapters for the European and the Asiatic sections but there is none for the entire war. The maps and pictures are carefully selected and extremely useful.

The major drawback of this otherwise excel-

lent account is the lack of footnote references. There are a number of statements for which citations to proper authorities would have been helpful. At the same time, the necessity to summarize complicated developments leaves the reader occasionally at a loss regarding additional details. Here, too, proper footnotes would have been worth the additional effort and space. Beyond that, the present volume is, without a doubt, the best general account of the Second World War.

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JAMES BARROS, editor. *United Nations: Past, Present, and Future*. New York: Free Press. 1972. Pp. 279. \$8.95.

ANDREW W. CORDIER and WILDER FOOTE, selected and edited with a commentary by. *Public Papers of the Secretaries-General of the United Nations*. Volume 2, *Dag Hammarskjöld, 1953–1956*. New York: Columbia University Press. 1972. Pp. xiv, 716. \$22.50.

SHIRLEY HAZZARD. *Defeat of an Ideal: A Study of the Self-Destruction of the United Nations*. Boston: Atlantic-Little, Brown. 1973. Pp. xv, 286. \$8.50.

The three books under review zig zag across the various levels and perspectives from which world politics may be approached. The essays edited by Barros focus on the institutions and activities of the United Nations; the Hammarskjöld papers cover a wide range of subjects as viewed by the second secretary-general of the United Nations; Mrs. Hazzard's monograph assesses the organization from the standpoint of the "second" United Nations—that is, from its most populous and widespread branch, the Secretariat. Together these books give us a picture of the personality, bureaucratic, nation-state, and system-wide factors at work in international organization since World War II.

Professor Barros has brought together six original essays that treat the past, present, and future of three institutions—the Security Council, the General Assembly, and the secretary-general—and the role of the UN in decolonization and in the development of international law, as well as its economic, social, and technological activities. Of uniformly high quality, these essays provide an ideal introduction to the subject for graduate students or for special-

ists on world affairs who wish to brush up on recent thinking and developments in international organization. The collection is sufficiently detailed that it could stand also as a reference book. Each essay contains a compact history of relevant events prior to 1945, a careful analysis of major trends since that time, and a brief outline of future alternatives. The essays on decolonization and social-technological activities are particularly interesting. Taken as a whole, however, the collection does not contain sufficiently new data or fresh interpretation to deflect the specialist from reading *International Organization* and other scholarly journals.

Leon Gordenker's essay in Barros on the secretaries-general reminds us of the alternating roles played by such men since the first days of the League of Nations and the International Labor Organization, ranging from overt activism to behind-the-scenes facilitating. All three books agree that Dag Hammarskjöld managed to combine both activism and quiet diplomacy to make his office an important and independent force in world politics. The public papers of Dag Hammarskjöld, therefore, are important sources not only for understanding the UN Secretariat but—even more so—for comprehending international organization in this period (1953–56 are the years covered in this volume). Most of these papers are scattered and hard to obtain, making this collection a must acquisition for university libraries. Cordier and Foote continue the high standards that they set in editing the public papers of Trygve Lie, not only in their selection of materials but in their competent introduction to the entire book and their historical notes giving the context of the selections, which include key reports, statements, addresses, verbatim transcripts, diplomatic communications, and press material of public record.

The most spirited of the three books reviewed here is Shirley Hazzard's *Defeat of an Ideal*, which makes a valuable antidote to the pious tone of the Hammarskjöld papers. Whereas Cordier and Foote served in positions close to the secretaries-general, Mrs. Hazzard worked for over a decade in less exalted niches within the Secretariat. She therefore saw the "other" UN from the ground up, rather than from the top floor. She provides, at a minimum, a heuristic corrective to the official line. Was Hammarskjöld

firm in resisting McCarthyite pressures from Washington to check out and perhaps force out certain U.S. nationals from the Secretariat? Did he spend considerable effort in molding the Secretariat into an effective bastion from which he could mount struggles to establish peace at Suez, in the Congo, and elsewhere? Was he a self-effacing man, catapulted into power but always retaining his own humility and sympathizing with the rank-and-file workers of his home base at Turtle Bay? To these and other questions we gather sharply differing conclusions if we read Hammarskjöld's public statements (or the Cordier-Foote commentaries) and Mrs. Hazzard's analysis.

Mrs. Hazzard holds that the paralysis of the UN—even its aging bureaucracy—may be traced to the failure of Lie and Hammarskjöld to stand up to the United States and other powers that sought to influence the selection of Secretariat personnel. She probably exaggerates the novelty of her charges (which she bases not only upon her own experiences in the Secretariat but also on the revelations of the McCarran and other congressional hearings), for the press conferences recorded in both the Lie and the Hammarskjöld papers show that they and working journalists were quite conversant with the political pressures exerted by the United States and other countries. Whether the secretaries-general practically killed the UN by submitting to FBI security checks for U.S. nationals is debatable. Pressures of this kind were known in the League period and continue to be exerted in various ways by the countries that present lists of candidates for Secretariat employment. A long half-life might in any case be preferable to a sudden demise. The perfidy of the secretaries-general—even if it could be established—would hardly be tantamount to the "self-destruction" of the United Nations, as implied in Hazzard's subtitle. Nor is it clear how "public opinion"—her *deus ex machina*—could ever be mobilized to rejuvenate the UN.

Mrs. Hazzard agrees that Hammarskjöld was the greatest of the four secretaries-general the UN has known. His public papers will occupy three volumes of the series edited by Cordier and Foote, compared with one for Trygve Lie, who occupied the position for a similar period. We need both the public position and critiques such as Mrs. Hazzard's to fathom the depth and

grasp the breadth of the man. We need not only the public image but also the recollections of those who knew him personally and who had to work with him. His own notes, *Markings* (1964), must also be evaluated. Were they the private or—as Mrs. Hazzard suggests—also the public Hammarskjöld? Here is material for a Bergman film, with the difference that the protagonist is clearly a giant among statesmen. Even his public papers display a deep knowledge of poetry in English, French, and German as well as Swedish. They reveal the genial mind invited to address the Museum of Modern Art, finding meaningful analogies between the problems of the “modern” artist and the struggles of the international civil servant. Hammarskjöld’s pride, to be sure, may have been his most serious fault. Like the mystics he revered (clear even from his self-analysis on an Edward R. Murrow program), he talked openly about the dangers of *hubris*, though he often acted as though he were possessed by it; withal he wore his hair shirt close to the skin.

WALTER C. CLEMENS, JR.
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ANCIENT

MASON HAMMOND, assisted by LESTER J. BARTSON. *The City in the Ancient World*. (Harvard Studies in Urban History.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xiv, 617. \$20.00.

Although historians have traditionally focused their attention on cities as creators and transmitters of civilization, urban history has only recently come into its own as a specialized field of study. It is therefore of interest that this book appears in a series devoted to the nature of cities and to the character of urban life throughout human experience. The book is also intended to fill a gap, since no general survey exclusively devoted to ancient urbanism exists in the modern scholarly literature.

Professor Hammond defines a city as an organized community exploiting its resources so as to create the economic base for political expansion beyond its immediately supporting hinterland. Concerned only with tracing the rise and spread of cities in areas having contact with the Middle and Near East and the Medi-

terranean basin, he takes as his basic chronological limits the emergence of the first cities in Sumer about 3200 B.C. and the end of classical antiquity, dated to the mid-sixth century A.D. The book naturally falls into three roughly equal parts dealing with the Near East and the Greek and Roman worlds.

He believes that the rise of Sumerian cities was at least in part a response to the challenges of organization posed by the creation of large-scale irrigation projects. Thereafter, urbanism developed widely in the Near East, normally as a result of local economic and political conditions but never without contact with older cities. Direct Sumerian influence, it seems, was extremely important for what became an abortive development of cities in the Indus basin; in Egypt urbanism was a rather late by-product of the political unification of the Nile valley. The author quite rightly insists that the widespread disruptions of civilized life in the Near East at the end of the Bronze Age did not, generally speaking, destroy its urban base.

Such was apparently not the case in the Aegean basin. The author tentatively concludes that there the Minoan-Mycenaean tradition was completely broken and that nascent Greek urbanism emerging in the early Iron Age was an independent Hellenic phenomenon made possible by a return to settled times. Greek life in cities, however, was soon influenced by the older urbanism of Asia Minor and the Levant. He regards as the most original Hellenic contribution to urbanism the invention of the city-state with its characteristic organs of government, magistrates, council, and assembly, and with sovereignty residing with the citizenry, no matter how narrowly defined.

Professor Hammond believes that the urban transformation of Rome depended on initial Etruscan domination but more importantly on Rome’s “historic role” in controlling central Italy. Seen in this light, Rome does not emerge as a true city until shortly before 300 B.C. Although affected by mature Hellenic institutions Roman urbanism was characteristically more primitive, since it never abandoned a preurban organization of the citizenry into tribes and kinship groups. The author emphasizes the most original Roman contribution to urban development as being the practice of dual citizenship; an ever-widening circle of her sub-

jects received Roman status while continuing active service as citizens of largely self-governing municipalities.

Such local autonomy was gradually limited, however, and then all but destroyed during the great crisis afflicting the principate during the third century A.D. Nevertheless, cities in general weathered the crisis and served the Late Empire as centers of civilization, although shorn of any real claim to self-government. In Western Europe urban life continued beyond Roman times until the eighth century, but municipal spirit did not survive to stimulate the general revival of urbanism that began around the year 1000. The author believes that there was greater continuity of the late ancient urban tradition in the Byzantine East and even to some degree in the various Moslem successor states that gradually absorbed it.

Despite a certain tendency to repetition this is a first-rate work of scholarship. Broadly conceived, it has the sweep of a handbook; maps, chronological surveys, an annotated bibliography of 160 (*sic*) pages, and indexes admirably support the text. Although specialists will find much to think about in this volume it will be most useful for readers who approach ancient urbanism with only a rudimentary knowledge of ancient history, since the general historical framework is sufficiently articulated to explain ancient cities to the beginning student.

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CHRISTIAN FROIDEFOND. *Le mirage égyptien dans la littérature grecque d'Homère à Aristote*. (Publications universitaires des lettres et sciences humaines d'Aix-en-Provence.) [Aix-en-Provence:] Ophrys. 1971. Pp. 403.

J. R. HARRIS, editor. *The Legacy of Egypt*. 2d ed.; New York: Oxford University Press. 1971. Pp. x, 510, 24 plates. \$10.00.

Greek literature has much to say about Egypt, albeit, not about the civilization studied by modern scholars but about a wondrous Egypt, the oldest of nations and the mother of civilization. From Homer to the end of antiquity writers increasingly elaborated the idealized portrait of Egypt as the country where wise priests preserved the wisdom of the ages in the mysterious hieroglyphs, priests at whose feet the

philosophers of Greece had sat and studied. Although a study of this aspect of Greek thought has long been a desideratum, complete treatment of the theme would be a herculean task as there is scarcely a Greek author whose works would not be to some degree relevant. Froidefond has made a beginning, however, with this thorough study of the references to Egypt in Greek literature from Homer to Aristotle.

Le mirage égyptien is in many ways a notable work. Froidefond's command of the extensive ancient and modern literature relevant to his topic is impressive. Illuminating observations abound, and a number of difficult problems have been significantly clarified by him. Notable in this regard is his demonstration that Homer's various references to Egypt are all compatible with conditions in the eighth and seventh centuries B.C. His vigorous defense of the originality of Herodotus's account of Egypt against those who would ascribe most of the best things in it to Hecataeus of Miletus is equally welcome. And yet, despite its virtues, this is an unsatisfying book. The problem lies deeper than the devotion of much valuable space to the discussion of obsolete theories. Put simply, Froidefond's approach to his subject is literary, not historical. He is not concerned to provide a reasoned account of the development of the Greek image of Egypt, but to determine how eight Greek writers—Homer, Aeschylus, Herodotus, Euripides, Aristophanes, Isocrates, Plato, and Aristotle—reacted to two stimuli: their often limited knowledge of Egypt and the intellectual currents of their own times. To achieve this goal Froidefond allots to each author a separate chapter in which a brief analysis of the state of Greco-Egyptian relations in his time is followed by an exhaustive analysis of the references to Egypt in his works. No general theme, however, unifies the book. Instead, the reader must thread his way through discussions of detail—valuable discussions, to be sure—guided only by vague references to the *mirage* and the significance of this or that aspect of the various authors' works to its development. Scholars interested in the authors treated or in the cultural relations between Egypt and Greece in the period before Alexander will profit from consulting Froidefond's study, but it is not the history of the Greek image of Egypt that its title seems to promise.

The Legacy of Egypt replaces in the well-known Oxford series the volume of the same title edited by S. R. K. Glanville. This is not, however, a revision of the earlier work, now over thirty years old but, except for C. R. Roberts's chapter on Greek papyri, a completely new collection of essays by some of the leading figures of contemporary Egyptology. A number of the pieces are of outstanding interest, notably Erik Iversen's study of "The Canonical Tradition" and its relation to archaic Greek sculpture and P. L. Shinnie's eminently sane assessment of Egypt's "Legacy to Africa." The primary significance of the new collection, however, is the picture it affords of the current state of Egyptology. Comparison with the first *Legacy of Egypt* is encouraging. Every chapter reveals an appreciable increase in the data at our disposal and, more importantly, an increasing understanding of that data in Egyptian terms. Space allows for only one example, but it is a revealing one. In seeking to account for the precocious knowledge of some aspects of human anatomy possessed by Egyptian doctors Warren Dawson, in the Glanville volume (pp. 187-88), pointed to the possibilities afforded by the practice of mummification for the study of the human anatomy. In the new *Legacy of Egypt*, however, J. R. Harris (p. 125) cites the evidence showing that Egyptian anatomical ideas were, in fact, derived primarily from animals. Harris is to be thanked for putting together this fine collection of essays, which should serve this generation of students as a convenient and reliable summary of our knowledge of Egyptian civilization as well as the old *Legacy of Egypt* did the last generation of students.

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T. B. L. WEBSTER. *Potter and Patron in Classical Athens*. London: Methuen and Company; distrib. by Barnes and Noble, New York. 1972. Pp. xvi, 312. \$17.50.

Professor Webster's admirable industry of scholarship is focused here on some pleasant topics in the field of Greek vase painting. Starting from the famous lecture given thirty years ago by the late Sir John Beazley, "Potter and Painter in Ancient Athens," Webster explores the links among potters, painters, and custo-

mers, and tries to account for the fluctuations in popularity of particular scenes. This endeavor requires enormous knowledge, not only of surviving vases, but of history and prosopography, musical, dramatic, and athletic contests, and cults and myth. There are few other scholars with the detailed knowledge to undertake such a broad survey.

Attractive ideas appear at many points. Webster suggests that the gilded youthful aristocracy of Athens, especially before the Persian Wars, commissioned vases with special scenes and inscriptions. An expensive symposion might be marked by orders for matching winebowls and drinking cups, with the names of the guests inscribed; after memories of the party faded, the vases might be sold secondhand in the western markets of Etruria. Athletes and concert artists might have sold their prize vases, and married couples their wedding gifts. There is no ancient body of literature on the topic of recouping personal finances by selling gifts and prizes, but the idea is more practical than one that involves cultured Etruscans commissioning vases inscribed with labels or *kalos* names of known Athenians.

The book has more to offer—it is a general survey of the fluctuation of popular scenes from about 550 to 400 B.C., with massive statistical tables and bibliographical references. Unfortunately this scheme makes the book very hard to read. Large clumps of pages are solid numbers from Beazley's *Attic Black-figure Vase-painters* (ABV) or *Attic Red-figure Vase-painters* (ARV), reorganized by themes—what one might term a cultural index to Beazley. Of course Professor Webster has clarified distinctions in Beazley's own very efficient indexes, separating lion hunts from hare hunts or women washing from women spinning, and between lists are stimulating suggestions—for individual flute performances as sparks for sudden outbreaks of certain mythical images or how poetic quotations got selected for the scrolls in school scenes. Yet this is not a book to read so much as a reference source for valuable and interesting cultural history, strongly documented. As we look forward to a generation of gleanings in Beazley fields, we are reminded that one of Beazley's attributes was his mastery of English prose.

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MEDIEVAL

ELEANOR DUCKETT, *Medieval Portraits from East and West*. Ann Arbor: University of Michigan Press. 1972. Pp. 270. \$10.00.

Miss Duckett, professor emeritus of classics at Smith College, has written another fine book, neither difficult nor especially profound, "simple pages" for the student or general reader. As in her *Carolingian Portraits* (1962) the structure is biographical, but now Miss Duckett begins with late antiquity, the barbarian invasions, and the dynasty of Theodosius the Great. Theodosius himself appears first, then in successive chapters persons of his house or its time, in each case paired man and woman: St. John Chrysostom, for example, with the Empress Eudoxia who brought him low; Synesius of Cyrene with his teacher the pagan philosopher Hypatia; Theodosius the Younger with his formidable sister Pulcheria. Then Miss Duckett moves, somewhat incongruously, to the ninth-century West, to Bernard of Septimania and his wife Dhuoda, and at last, inevitably, to Abelard and Héloïse, whose story does bear retelling in Miss Duckett's pretty style.

Some of the figures she includes do not lend themselves so well to this manner of treatment. The Emperor Honorius hardly figures in Miss Duckett's account of his reign, a traditional narrative concentrating on more important persons. Nor can Bernard and Dhuoda be disengaged in flesh and blood from the woeful history of Charlemagne's successors. Biography loses its advantage over less personal techniques when characters remain ciphers. Miss Duckett seems, moreover, to be less at home in late antiquity than in later periods. Her bibliography, admittedly "selected," omits recent works on precisely the figures she treats: major books of V. A. Sirago and S. I. Oost, for example, on Galla Placidia. At times greater care with the details of law and institutions might have added life and color. On July 4, 414, the reader finds, Pulcheria was "proclaimed not only Augusta . . . but also Regent for her brother." This is not in the sources. The Roman monarchy did not admit a formal regency, much less that of a female. Deprived of constitutional status the Theodosian women ruled the Empire by dominating their men.

In a work of this sort, however, these are minor blemishes. To the reader's profit Miss Duckett has worked extensively with the

sources and often allows her people to speak in their own voices. Chrysostom fulminates here against the whole race of women, while Synesius declares a disciple's admiration for Hypatia: "You, my mother, my sister, the teacher of my mind." The passages have been chosen judiciously and gracefully translated, adding to the charm of an attractive book.

KENNETH G. HOLUM

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JOHN W. BALDWIN and RICHARD A. GOLDTHWAITE, edited with an introduction by. *Universities in Politics: Case Studies from the Late Middle Ages and Early Modern Period*. (The Johns Hopkins Symposia in Comparative History.) Baltimore: Johns Hopkins Press. 1972. Pp. 137. \$8.50.

Mindful of the interest in university history generated by their own university's brief involvement in politics in 1970, the department of history of Johns Hopkins University initiated the project that resulted in the present collection of "historical case studies of the involvement of universities in politics" during the Middle Ages and the early modern period.

Unfortunately, some shortcomings in the work are noticeable. The introduction repeats unwarranted generalizations, such as that "the eleventh and twelfth centuries witnessed an awakening of medieval society after a long epoch of chaos and stagnation" (p. 3); that "in response to the national aspirations of large populations, territorial monarchies appeared, first in England and then in France, in the twelfth and thirteenth centuries" (p. 11); and that "exclusive admission to membership" was a privilege guarded by the universities "with hypersensitivity" (p. 8). Apparently, my own *Scholarly Privileges in the Middle Ages*, published by the Mediaeval Academy of America in 1962, was not consulted on the subject of university privileges. Disregard of the above work is also evident in the essay by J. K. Hyde on "Commune, University, and Society in Early Medieval Bologna," where much the same ground is covered in different form, to be sure, as that of chapters 1 and 2 of *Scholarly Privileges*. Moreover, Jacques Verger, in the essay entitled "The University of Paris at the End of the Hundred Years' War," although he actually encompasses the years from the early fifteenth century onward, deals with matter

presented in greater detail in *Scholarly Privileges* (pp. 182–226), but with a different emphasis. The account by M. Verger seems slanted against the university. This is shown for example in his assertion that the university, “after presenting the defense of Rouen as an issue of national solidarity . . . reconciled itself to the surrender of that city without any qualms” (p. 52), a statement that the records do not justify.

The third essay by Howard Kaminsky on “The University of Prague in the Hussite Revolution: The Role of the Masters,” is, on the other hand, an interesting and solidly constructed account of the political role of the university masters that “contrasts interestingly with the religious and non-national orientation of the University of Paris at this time” (p. 85).

The final essay, that of Christopher Hill on “The Radical Critics of Oxford and Cambridge in the 1650s,” provides an illuminating study, based on contemporary writings, of the objections to the universities made by both those within and outside the university. Among the objectives that have a familiar ring, enunciated, according to Professor Hill, by “many junior M.A.’s of Oxford—of the same age as today’s undergraduates,” were “more university democracy and reform of the curriculum” (p. 131). On the other hand, the seventeenth-century radicals outside the university wished primarily to separate the universities from their function of training students for the ministry.

These essays confirm the impression that university scholars were drawn willy-nilly into the political arena and that their writings provide an excellent source of information for the ideas and forces operative in their time. If the above collection serves to direct attention to these all too frequently neglected records, it will have served a very useful purpose.

PEARL KIBRE

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PETER CLEMOES *et al.*, editors. *Anglo-Saxon England*. Volume 1. New York: Cambridge University Press. 1972. Pp. xii, 332. \$19.50.

In the preface to volume 1, Peter Clemoes states the purpose of *Anglo-Saxon England*: “This new periodical . . . expresses the growing sense of community among scholars working in the various branches of Anglo-Saxon studies in many parts of the world. It reflects their realiza-

tion that the different disciplines . . . aid each other and are but aspects of a common interest. . . . This is the only regular publication devoted solely to Anglo-Saxon studies and to fostering cooperation between them all. . . . By bringing different specializations into direct communication we hope to promote fresh areas of knowledge and to invigorate growth in new directions.”

The articles fulfill the interdisciplinary purpose expressed above. In the first, “The Pre-Viking Age Church in East Anglia,” Dorothy Whitelock demonstrates her remarkable command of all possible sources in reconstructing an establishment completely wiped out by the Vikings. Not only do no pre-Viking sources survive from East Anglia, but the break in tradition was so complete that post-Viking sources are practically worthless. Whitelock must therefore put together a bit from Bede here, a sentence in a saint’s life there, a notation in a Continental manuscript, and so on. Yet her conclusion “that the church in East Anglia was not behind that in most of the other kingdoms, and that it was influential outside its own borders” is convincing. An appendix gives the episcopal succession for East Anglia from the beginning until the Viking invasions. Mary Anne O’Donovan also deals with episcopal dates in her “Interim Revision for the Province of Canterbury, 850–950, Part I.” Tables for each see are followed by notes, some of them quite extensive, on many of the names in the tables.

Janet M. Bately’s “Relationship Between Geographical Information in the Old English Orosius and Latin Texts Other than the Orosius,” taken with her study of classical sources in *England Before the Conquest* (1971), reveals a remarkable number of additions to the basic source. As Bately points out, at least some of these additions may rest on an annotated Orosius or a commentary. If they do not, they indicate the existence of an extremely painstaking scholar in a better library than one would expect in England around 900.

In “The Origin of Standard Old English and Aethelwold’s School at Winchester” Helmut Gneuss suggests that this standard was deliberately developed under Aethelwold’s direction. The thesis rests largely on the vocabulary of certain Winchester texts and, as Gneuss admits,

must stand the test of linguistic studies yet to be made; but he makes a good preliminary case.

Michael Lapidge edits three poems from the Cambridge University Library Manuscript KK. 5.34, 71^r–80^r. He assigns these poems to Winchester in the mid-tenth century on the basis of similarities to dated poems of known authorship. These poems are further evidence of some cultural continuity through the First Viking Wars, for their style is that of the southern English eighth century, just as English characters preserve the prose style of that school in the tenth century.

Kemp Malone's "Beowulf the Headstrong," which deals with Beowulf's ignoring both Higelac's advice not to fight Grendel and his retainers' advice not to fight the dragon, is a salutary antidote to the view that Beowulf brings ruin on the Geats through "avarice" for the dragon's gold. The ruin would have occurred "whether Beowulf died of a dragon's bite a little earlier or of old age a little later."

Literary criticism is represented by four articles: David Hamilton's "The Diet and Digestion of Allegory in *Andreas*," John F. Vickrey's "Exodus and the Treasure of Pharaoh," Daniel G. Calder's "The Vision of Paradise: A Symbolic Reading of the Old English *Phoenix*," and Paul Szarmack's "Three Versions of the Jonah Story." The first three, as their titles suggest, are examples of what might be called the exegetical school of criticism. The fourth is a study of narrative technique in Anglo-Saxon prose. Following these articles are Herbert Dean Merrit's ingenious and on the whole convincing conjectures on twelve difficult Old English words.

There are two paleographical articles. In the first, "The Manuscript of the Leiden Riddle," M. B. Parkes concludes after a most meticulous description of the manuscript that the riddle was copied after the rest of the text, probably at Fleury in the tenth century. "Northumbria and the Book of Kells" by T. J. Brown, with an appendix by C. D. Verey, illustrates the application of three specialties (paleography, archeology, and textual criticism) to a single problem. At one time the tradition of the great insular gospels—Durrow, Durham, Lindisfarne, Echternach, and Kells—was thought to be rooted in Ireland; but Brown places Durrow

in Northumbria and Kells in a center under Northumbrian influence, so that the flow of influence is reversed. The evidence is voluminous and rather complicated, and a good deal of ink may be used in the future on this subject; but I find Brown's thesis convincing in its main outline.

In "The Icelandic Saga of Edward the Confessor" Christine Fell presents a very neat study in intermediate sources. The saga uses material from the *vitae* by Osbert of Clare and Ailred of Rievaulx, but by way of an English service book. Material from Geoffrey of Monmouth's *Gesta Regum* probably came via the *Speculum Historiale* of Vincent of Beauvais, and a miracle involving SS. John and Edward very likely came from a collection of John's miracles. On the basis of the use of the *Speculum*, Fell dates the saga in the fourteenth century.

The two last articles deal with architecture, but in very different ways. In "Structural Criticism" H. M. Taylor suggests rules for a more systematic study of Anglo-Saxon buildings and appends one list of buildings for which there is some documentation and another of buildings that may show construction at two or more dates. In "The Anglo-Saxon House" P. V. Addyman sums up present knowledge of Anglo-Saxon domestic architecture. Until recently the picture was one of unrelieved squalor, since almost nothing but sunken huts had been discovered. At last, however, parallels to Continental "long houses" are coming to light, as well as two royal halls. Probably the sunken huts were often ancillary structures such as workrooms or storehouses. Where huts appear in Roman *insulae*, as at Canterbury, Roman structures may have served as dwellings. Yet excavation over rather large areas sometimes reveals only huts. Perhaps the first settlers had to be content with huts until they could build something better. Whatever their purpose, these huts explain the Germanic preference for gravelly, well-drained sites. Even in a workroom, a foot or two of water on the floor is inconvenient.

The bibliography with which the volume ends is to be continued annually. *Anglo-Saxon England* is off to a good start. Long may it continue.

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D. E. GREENWAY, editor. *Charters of the Honour of Mowbray, 1107-1191*. (Records of Social and Economic History, new series, volume 1.) New York: Oxford University Press for the British Academy, 1972. Pp. lxxxiv, 307. \$29.50.

A point Sir Frank Stenton made in *The First Century of English Feudalism* (1932) was that charters are a prime source for Anglo-Norman social history. Only a fraction of the vast amount of available manuscript material has been edited, even counting royal charters, so our view of English feudal society still remains very incomplete. The Greenway volume is a welcome addition of new information.

With a *firma* over £365 in 1130—a conservative indication of its worth—the Mowbray honor was of major importance and deserves the attention of social historians. The lords are also personally interesting, not seignorial abstractions. Nigel d'Aubigny, for whom Henry I created the honor, was one of the king's "new men" whose baronial status was achieved by his entry into royal service. His son Roger's career shows how the political stances of barons in Stephen's and Henry II's reigns might be influenced by the manner in which estates they claimed had been acquired or lost at Henry I's hands.

There is more to the charters than "meadow and moor." Two charters (nos. 116, 375) reveal how a tenant might best his lord over the reciprocal obligations involved in tenure. Roger de Mowbray granted a carucate by charter to Rainald de Mildeby for one-fifteenth of the service of a knight until he, Roger, performed some service that he owed Rainald. Rainald achieved this advantage because previously he had obtained a charter from Roger defining what was due him. Evidently upon Roger's satisfaction of Rainald, the tenant returned both the carucate and the charter to Roger. Administrative history is very well served. Some executive role in the household is indicated for the chaplain, Guy. There is a variety of material on the responsibility of the steward for general administration.

In the introduction it is curious that no over-all estimate of the Mowbray honor's value is attempted. A more serious query, however, can be directed at the editor's claim that the clerical staff of the Mowbray household developed into a chancery during the second half

of the twelfth century. There is no discussion of scribal script or of the size of the *acta* and little concerning formulas and drafting techniques upon which to base such an assertion. The illustrations of the two hands of household scribes found in the charters are of little use to paleographers. One can hardly say that a chaplain was a salaried professional simply because there is no evidence of how he was compensated.

Scholars will appreciate the carefully edited texts and useful indexes. Minor criticisms aside, Miss Greenway's edition of the Mowbray charters launches the British Academy's new series, "Records of Social and Economic History," with distinction.

ROBERT B. PATTERSON

University of South Carolina

ELSA DE HAAS and G. D. G. HALL, edited for the Selden Society by. *Early Registers of Writs*. (Publications of the Selden Society, volume 87.) London: Bernard Quaritch, 1970. Pp. cxli, 334, 334, 335-430. £7.00.

Writs were the official letters of medieval England sent by persons in authority to give notifications or to transmit their commands. The king and his officers were the greatest writers of writs, and because in the late twelfth and thirteenth centuries the royal administration became a bureaucracy handling large masses of routine business, the men who did the king's work designed standard forms of writs to take care of recurring cases. Most of the standard forms were for writs that directed the machinery of justice: orders to summon defendants, commissions to judges, instructions for bailing prisoners, and others in increasing profusion. Those who worked with the law began to keep books of forms, "registers of writs." The earliest that survive, from the 1220s, contain fifty-odd forms. Through the centuries that followed the growth of the legal system produced in 1531 the modern printed *Registrum* with about 2,500 entries.

No two manuscript registers are exactly alike. In the volume under review the editors offer a selection of five registers to represent the first hundred years of the development as we know it: two registers of the 1220s, one of the 1250s, another of the 1260s, and a final example from the years 1318-20. Each text

is taken from a single manuscript, but where the manuscripts make bad Latin or bad sense, as they very often do, the editors have emended them, sometimes merely correcting a verb form, sometimes reconstructing an entire writ that was garbled beyond comprehension by the medieval copyist. The editors' learning and judgment never seem to fail; writ after writ shakes off its errors and stands forth in a clear and convincing text. The English translations that accompany the Latin are made to the same standard, an unusually fine blend of accuracy and felicity. Elaborate pains have been taken to make the material accessible; in addition to the alphabetical indexes there is a concordance of the five registers and an analytical index which is itself a work of scholarship.

After a short general introduction by Miss de Haas, Mr. Hall's longer commentary presents his careful thoughts upon the historical problems that gather about the registers. He addresses the old problem of whether, behind the endless variety of the manuscripts, there lay an authoritative official register kept in the Chancery. He believes that there must have been such a thing; and in the early thirteenth century, he finds, it was a file of parchments, the medieval equivalent of a loose-leaf book. Again, he considers the origins of the classic distinction between original writs and judicial writs and concludes that the main lines of the distinction are visible even in the first years for which we have detailed knowledge of the writ system, around 1200. A good many other topics are taken up, invariably with the same fine balance of learning, imagination, and caution that informs this whole work.

DONALD W. SUTHERLAND
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MICHAEL PRESTWICH. *War, Politics and Finance under Edward I*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. 317. \$16.00.

Dr. Prestwich has presented us with a first-rate study of the interrelationships of war, finance, and politics under Edward I. He has thoroughly mastered his sources, especially those of the Wardrobe which provided the king with a clerical establishment for the conduct of war, and thus he is able to show us how Edward

waged war and just how expensive his wars were. These parts of the study are the most informative. But Prestwich also makes very useful observations on the political costs of Edward's wars, and the social and economic costs are considered, if only rather briefly at the end. It is natural in the work of a young scholar with an old subject that there should be a note of revisionism in this study, but it is scholarly and responsible. There emerges, for example, a convincing picture of Edward I as a domineering, rather slippery personality who brooked no opposition and reckoned no costs till he was driven to.

After an introductory narrative of the reign the study begins with an illuminating survey of the armed forces that Edward used. The cavalry included paid household troops, feudal magnates with their contractual retainues, and others, numbering some 3,000 in at least one campaign. The infantry were recruited in large numbers; 25,700 can be shown for one campaign, probably the largest English army before 1642. Victualing arrangements for these large armies were superior to those used thereafter until at least Elizabeth's reign or later. The navy was used in the victualing enterprise as well as in the coastal defense of England. The responsible officials, chiefly of the Wardrobe, were thoroughly competent, if also notoriously corrupt.

The costs of all these troops, their supplies, and fortifications was dear. For the period 1294-98 alone Edward's military expenses are estimated at £750,000. Since the normal Crown revenue averaged about £18,000 a year the king had to rely upon taxation. Over the whole reign the taxes on the movables of the laity are estimated at £500,000 and taxes on the revenues of the clergy at £300,000. This was heavy taxation but not nearly enough money to meet Edward's needs. The customs, export duties paid by merchants, were a major new source, but many other financial expedients had to be resorted to. Even so Edward was obliged to seek credit in huge amounts, since he seems never to have exerted control over expenditure; he did what he wanted to do and left it up to his ministers to pay for his orders as best they could.

"By the end of Edward I's reign the crown finances were in a chaotic state" (p. 222). This

financial chaos, in Prestwich's view, is the setting for the political conflicts of the later years of the reign, and the troubles of Edward II are seen as largely the result of his father's reckless extravagance. Some evidence is also given to suggest a breakdown of law and order in the later years of Edward I, which carried on into the reign of his son. The bad economic effects of Edward's policies are listed: the use of Italian merchants precluded the emergence of English financiers; the incidence of taxation and the export of money adversely affected English trade; purveyance and recruitment of soldiers, and most of all, taxation must have weighed heavily on economic production, especially agriculture. Although Prestwich does not draw any conclusion about the relationship between the consequences of Edward I's wars and the "depression" of the early fourteenth century described by Professor Postan, he does indeed suggest a connection that deserves further exploration.

FRED A. CAZEL, JR.
University of Connecticut

RICHARD W. KAEUPER. *Bankers to the Crown: The Riccardi of Lucca and Edward I*. Princeton: Princeton University Press. 1973. Pp. xviii, 279. \$12.50.

The Riccardi of Lucca were the first of a number of Italian commercial houses that, in the late thirteenth and the first half of the fourteenth centuries, served as bankers to the English Crown. In this book, originally a doctoral dissertation written at Princeton University, Richard W. Kaeuper examines what he appropriately calls the "Riccardi system" of royal finance, which was operative in England for some twenty-two years, until the firm lost the favor of King Edward I in 1294. The study is based chiefly upon a rigorous analysis of documents today preserved in the Public Record Office. Kaeuper first considers the structure and personnel of the Riccardi bank and the nature of their interests in moneylending and the wool trade. He then examines the changing character of the king's fiscal needs in the middle and late thirteenth century. Growing reliance on a paid army and bureaucracy and the construction of Welsh castles drove up the costs of war and government

beyond what the king's ordinary revenues could support. Edward energetically developed new sources of income by imposing taxes upon wool export and upon the movables of his subjects. These new fiscal recourses, while highly remunerative in the long run, still could not supply him with sufficient liquid funds in periods of crisis. Edward's reliance upon the Riccardi solved this problem of liquidity. From their own funds or from those borrowed from other Italian bankers the Riccardi advanced him liquid capital in large amounts. They were in turn given the right to collect or administer tolls and other sources of royal revenue. Kaeuper congratulates Edward on the success of the system, which, in the author's opinion, provided an essential fiscal support to the other major accomplishments of the reign. Although the Riccardi were disgraced in England in 1294 (for reasons still not entirely clear) and failed not long afterward, Edward and his successors continued to seek the aid of Italian bankers. The system proved its value and endured for another fifty years.

The strengths and occasional weaknesses of this study primarily reflect the character of the sources that support it. The chapters on the structure and personnel of the Riccardi bank are somewhat inconclusive and colorless; the large and still poorly known archives of Lucca itself would have to be consulted to enrich the picture. On the other hand, the description of the "Riccardi system" and its services for Edward I is excellent. The book offers, from a new perspective, many insights into English royal finances and Italian international banking in the thirteenth century.

DAVID HERLIHY
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JOEL T. ROSENTHAL. *The Purchase of Paradise: Gift Giving and the Aristocracy, 1307-1485*. (Studies in Social History.) Toronto: University of Toronto Press. 1972. Pp. xiv, 169. \$8.50.

Following the current interest in finding statistical answers to sociological and anthropological questions, Joel Rosenthal offers us a social study of the English aristocracy in the late Middle Ages. By focusing on a specific voluntary activity of giving gifts to the Church, he illuminates the general social behavior of

an important medieval class. Such a study can be best attempted in England because of the well-known advantages offered by the wealth of documentation. Full series of royal records enable him to isolate an important sample of the high aristocracy, the parliamentary baronage, so-called because of their individual summons to parliament by the king. Furthermore, such royal records enable him to study exhaustively their landed gifts to the Church because of the peculiar English mortmain legislation. (To alienate land to the Church one needed a special license from the king.) Using these sources the author has compiled solid and interesting statistics. But England also affords a good number of surviving wills pertinent to the subject. Here the author's endeavor lacks methodological clarity. We are not told how many wills are used, what their validity is as statistical samples, nor how their results can be combined with the other evidence. From these materials his study becomes more impressionistic, resembling the older investigations on which he attempts to improve.

Since valid generalizations are the chief justification of the statistical approach, what are the author's major conclusions? In a preliminary way he states that the family was the functional unit of aristocratic society and that nobles were strongly influenced by local ties. (Since the first was actually the working premise of the study, it cannot be confirmed until alternate premises have been explored.) But neither conclusion will change our long-cherished views of medieval aristocracy. More significant are Rosenthal's negative conclusions: that aristocratic benevolence waned toward the end of the Middle Ages, that the English aristocracy lacked class cohesion in this realm of behavior almost to the point of extreme individualism and idiosyncrasy, that they consciously renounced efforts of social manipulation in their giving, even in refusing to reinforce class distinction, and, most important, that they made no discernible effort to use their benefactions for political purposes. (Even the king refused to use his powers to control aristocratic benefactions.) Since this final conclusion is most surprising and stands at variance with what little we know about kings and nobility elsewhere, such as in France, the author has challenged future scholars to

pursue these studies to explain their discrepancies.

JOHN W. BALDWIN
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JOHN BELLAMY. *Crime and Public Order in England in the Later Middle Ages*. (Studies in Social History.) Toronto: University of Toronto Press. 1973. Pp. 229. \$10.00.

A century ago L. O. Pike wrote *A History of Crime in England*, but nothing further has appeared on the subject in the Middle Ages until now. In this book on crime in the fourteenth and fifteenth centuries Professor Bellamy uses the many legal records published since Pike's day and others yet unpublished. The seven chapters dealing with misdeeds and misdoers, criminal bands, enforcement of the law, accusation and trials, prisons, punishments, and pardons portray in great detail English medieval crime.

Although Professor Bellamy does not make the comparison, it is remarkable how similar crime was then to that of more modern times. Except for the reaping of greater profits in the Great Train Robbery and in the efficiently organized crime of the Mafia, the techniques seem hardly to have changed. Then, as now, crime flourished upon the connivance of those in government. The members of the royal household and highly placed officials not only abetted and profited from crime but occasionally participated in it. The dishonesty of such lesser officials as sheriffs, constables, gaolers, and even justices was notorious. In an age of retinue and maintenance it was practically impossible to bring the overmighty subject to justice. His indentured bands, often no more than criminal gangs, plied their occupation with impunity. No form of crime was unknown. It ranged from the more violent and serious homicide, murder, robbery, larceny, kidnapping, and rape to bribery, corrupt practices, confidence games, pimps, madams, and their hard-working prostitutes. How often canons, monks, friars, and priests were involved! And as more people became literate and could read some Latin the scandal of benefit of clergy grew. Fifteenth-century crime so impressed Chief Justice Sir John Fortescue that he ingeniously attributed it to "a rather praiseworthy determination not to be

overawed by the law, an independence of spirit which gave and took hard knocks in good part" (p. 31). Then, as now, those who enforced the law had to cooperate with those who broke it in order to bring some culprits to justice. Informing almost became institutionalized. According to Bellamy, "the approver became the agent of the king in preserving public order" (p. 130).

To compile exact data from medieval records is difficult, but Bellamy makes a good case for his assertion that crime and disorder rose alarmingly in the fourteenth and fifteenth centuries. Why? Because much depended upon the presence and the ability of the kings; there were some like Edward III and Henry V who were too often absent from the realm for long periods and those like Edward II, Richard II, and Henry VI who were ineffective. Then, too, the indenture system often blocked the functioning of the common law and the system of courts, while feuding among the great lords and civil war in the fifteenth century both contributed to disorder and obstructed those whose duty it was to apprehend criminals and to make the courts work. Bellamy believes that neither before nor since these centuries "has the issue of public order bulked so large in English history" (p. 1) and attributes it basically to the fundamental social and economic changes in a time of maladjustment and transition from a feudal and manorial society to one that increasingly was not.

While most of the conclusions are backed by sufficient evidence a few seem indefensible or require additional clarification. Contrary to Maitland, Bellamy has argued that most of those indicated did not object to jury trial because they were aware of local sympathy and of the high incidence of acquittals. An interesting point, but it requires further study. Bellamy notes and attributes the sharp rise in pardons granted by the kings to offenders to some understanding that such a practice could save many offenders from being irrevocably alienated from society and could prevent others being blighted by a single crime, but such understanding seems too enlightened for the age. Certainly the main reason for pardons was the royal need for money. Bellamy concludes that punishments for crime became less cruel and barbarous, but he makes no attempt to deter-

mine whether this was a reason for the increase in crime. This is a problem still debated by those in criminology and law enforcement. As for the high incidence of crime, especially in the fifteenth century, one could wish that Bellamy had related this phenomenon to the feeling of many scholars that the civil war and political disorder of the fifteenth century little influenced the daily life of the common man. Bellamy's evidence points to the contrary.

This book is important for students of English medieval legal, institutional, political, and social history who would perhaps welcome only more analysis.

BRYCE LYON

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ARTHUR J. ZUCKERMAN. *A Jewish Principdom in Feudal France, 768-900*. (Columbia University Studies in Jewish History, Culture and Institutions, number 2. Edited under the auspices of the Center for Israel and Jewish Studies, Columbia University.) New York: Columbia University Press. 1972. Pp. xvi, 490. \$20.00.

With a working knowledge of at least a half-dozen languages Professor Zuckerman has demonstrated a prodigious capacity for digging into an immense variety of sources that are rarely mined in a unified context. This labor is not only evidenced in twenty-eight pages of "Select Bibliography" but by hundreds of learned footnotes. Thus it is disappointing that our author has discovered pyrites and cast a grotesque in fool's gold. It is especially unfortunate that many precious pits and pieces of reliable evidence are so thoroughly confounded with the worthless that only the specialist in Carolingian history, after a careful rereading of the book, will be able to form a reasonable picture of the situation.

With a highly selective use of *chansons* and other literary materials from the twelfth century and later Zuckerman sets out to prove that a vast Jewish principality embracing much of southern Gaul and northeastern Spain was established and supported by the Carolingians in concert with the Caliph of Baghdad. Zuckerman maintains that a Jewish king was imported from the East and married to a Carolingian princess to secure this complex arrangement. Count William of Gellone and his large-nosed offspring are identified by Zuckerman as the

Jewish royal family descended from King David.

It is not possible to ascertain why Zuckerman abandoned the canons of historical method, or why he chose to accept as superior the evidential value of fictional literary sources far removed in time from the events under consideration and to relegate documentary evidence of a more contemporary nature to inferior status. This lapse is particularly puzzling because in a recent article on Agobard of Lyons Zuckerman demonstrates a firm command of everything that makes an excellent historian. It is tempting to speculate that Zuckerman's vigorous though implicit reaction against the "lacrymose interpretation" of Jewish history that dominates the textbooks and his disdain for "majority historians" who have neglected and degraded Jewish history overstimulated his imagination. More fundamentally, however, Zuckerman seems to have become the victim of two genres of source material from which it is very difficult to obtain the kind of hard historical evidence that he sought. Efforts to secure sound data about the course of events from *chansons* and from *responsa* frequently lead to the pyramiding of conjectures. Zuckerman indulges in this dangerous practice and then rejects more compelling evidence in light of his previous conjectures.

Our knowledge of Jewish history and of the early Middle Ages in general would have been better served by a plausible, straightforward, well-organized, and clearly written reconstruction of the history of the Jews in southern France and the Spanish March. Despite all of its shortcomings, however, Zuckerman's book is fascinating, a kind of footnoted romance that can be both enjoyable and profitable to the properly cautioned reader.

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LAURO MARTINES, editor. *Violence and Civil Disorder in Italian Cities, 1200-1500*. (UCLA Center for Medieval and Renaissance Studies. Contributions: 5.) Berkeley and Los Angeles: University of California Press. 1972. Pp. viii, 353. \$12.00.

The papers collected in this volume were first read at a conference in Los Angeles in May 1969, a time when violence both on campus and

in Southeast Asia was of deep personal and national concern to all but two of the contributors (Professors Larner and Hyde, who had come from England). "Like many of us," Professor Brentano begins, "I have recently suffered peculiar changes in my perception of violence." And it is worth saying at the outset that this personal concern extended the scope of the treatment of the subject, primarily by including the violence of punishment and control and by connecting the potential violence in a society, especially as evidenced by crime, with the organized violence of faction, revolt, and war, without maiming the enduring value of the papers through too heavy a dose of subjectivity. Moreover, "the history of violence is also a history of the values and institutions that define it," writes Professor Martines, who organized the conference, in his introduction, and though written after the event, this axiom underlay the papers as they were composed and gives the collection a pertinence to any student of northern and central Italy from the thirteenth to the fifteenth centuries.

His introduction, a model of dispassionate appraisal—for it does not avoid criticism of the methodology employed in some of the contributions—is followed by an opening after-dinner speech by myself, which was printed as an act of editorial courtesy but neither calls for comment nor is, I hope, indicative of any bias that might make the reader think twice about the following, necessarily bald account of the ten papers that make up the body of the collection.

In "Order and Disorder in Romagna, 1450-1500," Professor Larner tilts gently against the conference's emphasis on violence in cities; "here violence was almost pre-urban in character: it came from the world of the peasant and the aristocrat, it was the fruit of underdevelopment rather than overcomplexity." And in his review of conditions in Rimini, Cesena, Forlì, Imola, and Faenza, he is careful not to distinguish too sharply between the values and institutions of the towns and their *contadi*. He is particularly illuminating on the effects of clientage on a *signore's* freedom of action, the consequences of broken as opposed to continuous rule, and the need to balance protoclass manifestations of violence against the emotional temper of the region and the age. Professor

Ilardi, in "The Assassination of Galeazzo Maria Sforza and the Reaction of Italian Diplomacy," describes the diplomatic repercussions of the murder of a head of state in vivid if orthodox terms. It is the only paper that is chiefly concerned with the consequences, rather than with the motives and circumstances of an act of violence.

From the *Libro de' Giustiziati* in the Biblioteca Comunale Ariostea in Ferrara, Professor Gundersheimer analyzes crimes punished with death in "Crime and Punishment in Ferrara, 1440-1500." He sees no crime waves, save those associated with political revolts, no dramatic incidences of violent crimes, an increase in hanging at the expense of beheading and burning, and a "relative decline in the criminality of the Ferrarese people around the turn of the century." He recognizes that his source does not give a complete picture; his conclusions contain no surprises, but the rarity of such an analysis, coupled with the caution and neatness of his demonstration, give this paper a modest claim to real distinction. The deftness with which Professor Herlihy humanizes the statistics deducible primarily from the Florentine *catasto* of 1427 in order to show the criminal violence latent in a situation where men outnumbered women, married late, and were considerably more numerous than their wiser elders, will come as no surprise. In "Some Psychological and Social Roots of Violence in the Tuscan Cities" he finds that the predispositions to violence suggested by contemporary moralists fit the demographic evidence, but he leaves himself provokingly little space to deal with any form of organized group violence. Professor Brucker's "The Florentine *Popolo Minuto* and its Political Role, 1340-1450" achieves, with a remarkable blend of evocation and precision, three objectives: determining the effect of the *popolo minuto* on political-constitutional crises, describing the life-style of the poor, and comparing their criminality with that of the rich.

The sources most relied on by Professor Chojnacki in "Crime, Punishment and the Trecento Venetian State" (we are now moving backward in time, according to the editor's arrangement of the articles) are those dealing with the *signori di notte* and the criminal courts. With statistically buttressed dauntlessness he challenges the "magic wand" theory of

the *Serrata*, tackles the problems posed by the high number of transients and foreigners in Venice, and sees the eventual answer to aristocratic and lower-class violence in terms not too much at odds with the "myth" of evenhanded justice. Like Ilardi, Professor Bowsky, in "The Anatomy of Rebellion in Fourteenth-Century Siena: from Commune to Signory?" concentrates on a single moment of violence, the abortive revolt of 1318, but his intention, pursued with both strength and delicacy, is "to attain a more nuanced and accurate knowledge of the role and nature of conspiracy and rebellion in Italian society." Professor Hyde's "Contemporary Views on Faction and Civil Strife in Thirteenth and Fourteenth-Century Italy" moves from the explanations offered for violence (paralleling the initial approach of Herlihy) to examples of revolts recorded by chroniclers, proceeding by way of excellent definitions of terms like "commune" and "Ghibellini" that help give the volume as a whole a value beyond the sum of the crimes and crises it is chiefly concerned with.

Professor Brentano's "Violence, Disorder and Order in Thirteenth Century Rome" (we are still moving backward in time to an increased incidence of violence in all its forms) takes us as far south as we get and into the most elastic of governmental organizations. In part because of the fragmentary nature of the evidence, but also, I suspect, from choice, the treatment is (in the concrete sense of the word) phantasmagoric, and it does not eschew the occasional guess. But the humanity of its tone is impressive and its conclusion, that "the violence of thirteenth-century Rome may have been socially functional, or sociologically functional, but it must have been humanly destructive, even for the survivors," is convincing. Professor Martines ends the volume with a crisp account of the economic, demographic, political, and social causes of "Political Violence in the Thirteenth Century," which has, apart from its own summary value, the merit of encouraging the reader to rethink the problems explored in the work as a whole. The publishers have associated themselves with the spirit of the enterprise by supporting order (the footnotes are at the bottom of the page) and committing a crime (there is no index).

J. R. HALE

University College London

JOHN BELL HENNEMAN. *Royal Taxation in Fourteenth Century France: The Development of War Financing, 1322-1356*. Princeton: Princeton University Press. 1971. Pp. xvi, 388. \$15.00.

MARTIN WOLFE. *The Fiscal System of Renaissance France*. (Yale Series in Economic History.) New Haven: Yale University Press. 1972. Pp. x, 385. \$17.50.

These two volumes join the unusual number of books that have appeared in the last decade on French fiscal history of the medieval period and the *ancien régime*. Both deal with similar problems but differ strikingly in their purpose and treatment. Professor Henneman presents a most detailed and empirical study of war financing across a forty-year period. He has explored the municipal and departmental archives of much of France as well as the central depositories at Paris and seems to have uncovered every pertinent unpublished document. Professor Wolfe has written a much broader but still precise account of French fiscal resources and procedures across a two-hundred-year period, largely basing his work on a careful reading of published materials with occasional references to archival sources.

American scholars have a conditioned interest in the fiscal history of early European states for the connections that usually exist between the monarch's need of taxes and the rise of representative assemblies. The first of these volumes is filled with material on assemblies used by the first Valois kings: town, bailiwick, and provincial and regional assemblies. They played a less important role under the last Valois, who learned to tax without them. At both ends of the dynasty, unyielding local particularism was the ultimate obstacle to the creation of an Estates-General that could give or withhold consent to taxation on behalf of the nation. In the early years this particularism persisted in spite of the external threat posed by the Hundred Years' War. In the end, it flourished because of the centrifugal force of civil war. And in between, it seems to have been endemic with or without war.

War provided the *necessitas* that enabled the last Capetians and early Valois kings to ask for subsidies from their subjects. With the beginning of the Hundred Years' War in 1337, Philip VI and John II sought tax support annually for the next two decades. In studying this continuous negotiating, Mr. Henneman sees the

emergence of significant patterns of behavior. The north of France, Languedoc, was more easily persuaded to grant taxes, presumably because it saw the threat of invasion more clearly. It preferred to grant a sales tax while Languedoc usually chose direct taxation through the hearth tax. The south always offered greater resistance to taxation, even when the enemy was upon it, and the king resorted to commissioners, who, while seeking the requested subsidy, also investigated and fined for usury, money violations, and *franc-fief* acquisitions, and used other fiscal devices. Consent to taxation was usually sought in local and bailiwick assemblies, but after 1346 the Estates of Languedoc played an important role that signified the growing sense of separateness in the south. The evidence for these patterns is clear enough; we are sometimes left wondering why they developed—why the north, for example, preferred a sales tax and the south a hearth tax.

Unlike Mr. Wolfe, who can bring his fiscal ship into Sully's safe harbor, Mr. Henneman must leave his foundering on the rocks of the 1356 defeat and capture of the king. But he offers one last and significant thesis: as auspicious as the Languedoc Estates of 1355 was, its proposals, not only to consent to taxes but to collect them, ended in failure. Rather, it was the Poitiers disaster that introduced a new era in French fiscal history. Whereas taxation had heretofore centered on the war subsidy, the exigencies of John II's ransom would make the *aides*, *gabelles*, and *tailles* the basic royal taxes for centuries to come.

Mr. Wolfe's contrast of the sixteenth century with the fourteenth leads him to assert that the former, especially the age of Francis I, differed from the latter in three important respects: the appearance of a strong centralized treasury apparatus (the *Épargne*), the origins and growth of venality, and the power of the king to tax without the consent of the people. This last condition originated in the 1430s, in another age of crisis, when a series of grants of *aides* and *tailles* by several meetings of Estates led Charles VII to continue collecting these taxes every year without further consent. This tax "absolutism" of Charles VII led to the fiscal "absolutism" of Francis I, a phrase that I felt, several years ago, described Philip the Fair's reign. This development in Charles VII's reign makes it impossible to think of sixteenth-century

French tax history as merely a continuation of the late Middle Ages. The period from Charles VII through Henry IV can be considered as a single historical unit in French fiscal history.

In addition to this general view, Mr. Wolfe proposes a special thesis when he studies the role of taxation in prolonging the Wars of Religion. The system so carefully constructed by Francis I and Henry II made France wealthy in royal revenues, all flowing upward from the local level. With the outbreak of the religious struggle, this revenue system became one of the prizes sought by the warring groups. In the 1570s the Protestants controlled the royal tax machine of Languedoc and sought consent to taxation from Huguenot assemblies. But this was pure order compared to what followed in the late 1580s and early 1590s, when fiscal disobedience was rampant and France seemed on the verge of utter disintegration. Grandees, *gouverneurs*, and other lords controlled counties, cities, and regions, and their autonomy, existence, and fiscal rape depended on the continuation of hostilities.

Finally, in reclaiming the machinery of Francis I and Henry II, Sully inadvertently bound future generations, even into the eighteenth century, to the institutions and attitudes of the Renaissance system, "whereby a moribund autocracy tightened its suffocating grip on a great country."

Both historians have included valuable appendixes. Mr. Henneman provides a careful assessment of coinage alterations for a clearer view of the comparative value of tax grants. The last third of Mr. Wolfe's book contains eleven short essays on technical aspects. A twelfth essay on sixteenth-century monetary units would have been helpful. These two studies of Valois taxation belong with the best that has been written on French fiscal history.

FRANKLIN J. PEGUES
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G. L. KURBATOV. *Osnovnye problemy vnutrennego razvitiia vizantiiskogo goroda v IV-VII vv. (Konets antichnogo goroda v Vizantii)* [Basic Problems of the Internal Development of Byzantine Cities in the 4th-7th Centuries (The End of the Ancient Cities in Byzantium)]. (Leningradskii Ordena Lenina i Ordena Trudovogo Krasnogo Znameni Gosudarstvennyi Universitet imeni A. A. Zhdanova.) [Leningrad:]

Izdatel'stvo Leningradskogo Universiteta. 1971. Pp. 219.

Economic and social history of the late Roman and Byzantine Empires has traditionally attracted the interest of Russian Byzantinists. Modern Soviet historiography has continued and developed this trend, frequently making exaggerated use of Marxist categories and methodology. These strictly ideological exaggerations are fortunately rescinding in the work of most serious Soviet historians today.

G. L. Kurbatov has been well known since the mid-fifties as the leading Soviet specialist on the history of the Byzantine city. He is the author of a monograph on Antioch in the sixth century (Leningrad, 1962), which appeared almost simultaneously with a major American study on the same subject (Glanville Downey, *A History of Antioch in Syria from Seleucus to the Arab Conquest* [1962]), and of several important articles on the transformation of the old Greco-Roman city into a medieval "feudal" town. One must note, at this point, that the concept of "feudalism" is used by most Soviet historians in a much wider and less technical sense than is the case in Western historiography.

In his new book, which deals with the same general issue, G. L. Kurbatov studies the relationship between the economic life of city and countryside in the early Byzantine period, the decadence of many small cities, the emergence of large city-centers, the evolution of social structures in the urban population, and the disintegration of the old municipal institutions. The *Codes* of Theodosius and Justinian provide the author with his major primary sources of information, which are used with great intelligence and critical sense, together with such other evidence as may be available in Libanius, Synesius, Chrysostom, and the Byzantine historians.

Kurbatov's thesis on the radical disintegration of slavery, which was the basis of urban economy in the ancient "city," and its replacement by an economy of free enterprise was criticized in 1964 by his colleague, M. Ya. Syuzumov, but is fully maintained in his new book. The new development, in the author's opinion, is connected with such facts as the emergence of small industries, the social and cultural changes provoked by Christianization, and the new role of the Church, as well as with the disappearance

of the old "city's" independence in the framework of centralized imperial structures.

It is unfortunate that very few historians of late antiquity and medievalists have access to Russian publications. They would certainly profit from reading Kurbatov's study. Unlike that of many contemporary Soviet publications, especially on art, Kurbatov's book is anything but luxuriously presented. It is a paperback and, unfortunately, lacks an index. It is, however, a mine of factual information and a piece of very serious scholarship.

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nation that under the circumstances could only be performed in some indirect fashion. In Byzantium, although Dr. Stauridou does not agree on this point, coronation by the patriarch was indispensable, for it completed the process whereby one was fully vested with the imperial authority. In this instance coronation meant that Symeon was recognized as emperor—emperor, of course, of the Bulgars—but it meant also that the Byzantines recognized the existence of another empire. Herein lies the significance of the question studied by Dr. Stauridou.

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ALKEMENE STAUROIDOU-ZAPHRADA. *Hē Sunantēsē Sumeōn kai Nikolaou Mustikou (Augoustos 913) sta Plaisia tou Buzantinoboulgarikou Antagōnismou* [The Meeting of Symeon and Nicholas Mysticus (August 913) in the Context of the Byzantino-Bulgarian Struggle]. (Byzantine Texts and Studies, number 3.) Thessaloniki: Center of Byzantine Studies. 1972. Pp. 130.

This book concentrates on an important episode in the relations between Symeon, no doubt the most ambitious medieval Bulgarian king, and Byzantium. Symeon, who had previously defeated the Byzantine armies, again threatened Constantinople, now guided by a regency threatened by internal dissension. But he finally agreed to negotiation, and for this reason was allowed to enter the Byzantine capital in August 913.

A problem associated with these negotiations is the question whether they included the coronation of Symeon as emperor, to what extent was there positive agreement on this point, and how, if at all, was the act of coronation carried out. The sources are not quite clear and modern authorities disagree in their interpretation of them. This is the problem that Dr. Stauridou has attempted to elucidate.

One may say at once that in method Dr. Stauridou's book is an admirable composition, worthy of the high scholarly standards of her mentor. But her conclusions—that in these negotiations there was no question of any coronation and that what the patriarch did when he met Symeon was to give him his blessings—are open to doubt. Granted that the sources are by no means clear; nevertheless they do hint at some kind of coronation, a coro-

MODERN EUROPE

LAWRENCE P. BUCK and JONATHAN W. ZOPHY, editors. *The Social History of the Reformation*. Columbus: Ohio State University Press. 1972. Pp. xxiv, 397. \$12.50.

While Reformation studies have been languishing in Germany since the Second World War they have attracted a considerable number of young scholars in America. Harold J. Grimm, to whom his former students and American and German friends have dedicated this volume, is one of the historians who has stimulated this lively interest in Reformation history. As Grimm has been particularly interested in the social forces operative in the Reformation the seventeen essays of this volume deal with "the impact of the Reformation on some aspect of society in sixteenth-century Europe." The city of Nuremberg figures prominently in this book because Grimm has been especially interested in Nuremberg's role during the Reformation. The editors have arranged the essays in three categories: the municipal setting of the Reformation, the impact of the Reformation on society, and the organization of the Reformation. The essays are well researched, have good documentation, and deserve the attention of Reformation scholars.

In the first essay, on "The Control of Morals in Calvin's Geneva," Robert Kingdon argues that the stern austerity of Calvinism, particularly in sexual matters, resulted from the rigorous enforcement of morality by the consistory. Kingdon discusses the composition, election, and functions of the Genevan consistory in detail.

It is of interest to know that the vast amount of material emanating from this morals court has not yet been exploited.

In a skillful study of the beginnings of the Reformation in Nuremberg Gottfried Seebass shows that sociological categories such as popular movement or conciliar Reformation do not apply to Nuremberg. Humanist patrician circles were the first to accept Luther's doctrines which then also spread to the clergy and the ordinary burghers, with the evident approval of the town council. Between 1523 and 1533 the city government effected the decisive reforms, following the wishes of the mass of people. Charles E. Daniel has devoted an essay to one of Nuremberg's leading reformers, Wenzeslaus Linck. According to Daniel, Linck did not share the traditional negative view that work was the curse of mankind because of original sin. Work is good because it has therapeutic value for the individual and furthers the general welfare. Unlike Luther, Linck put his ideas on education in a theoretical framework, speaking of the child's mind as a *tabula rasa*. Philip Norton Bebb's essay is devoted to Dr. Christoph Scheurl, Nuremberg's well-known jurisconsult. Bebb offers a very instructive description of the three bodies of lawyers at Nuremberg, their functions, and the complicated court system. While the sketch of Scheurl's diplomatic missions in 1524 and 1525 tells much about Nuremberg's ambiguous religious policy it does not reveal much about Scheurl himself. (One note of caution: the free imperial cities were not sovereign. For example, in 1548 Charles V simply changed the constitutional setup of the free imperial cities in southwest Germany.) Jackson Spielvogel's essay on "Patricians in Dissension" discusses Willibald Pirckheimer's personal quarrels with other members of the Nuremberg town council. Spielvogel looks at these venomous fights from the point of view of the social historian, arguing that the town council made considerable efforts to patch up the quarrels among Nuremberg's ruling families. These feuds also prove that Nuremberg's famous humanist was indeed human.

Richard C. Cole's essay on "The Dynamics of Printing in the Sixteenth Century" argues that the appearance of thousands of books and pamphlets brought about a "change in the consciousness of society." The printed word

added a new dimension to Luther's revolution. As an example he points to the appearance of Eberlin von Günzburg's *model of a new society* and the enactment of laws on poor relief in the 1520s. Although a bit speculative the essay contains stimulating suggestions. The problem of a causal relation between the German Reformation and the Peasants' War of 1525 is the topic of Hans J. Hillerbrand's essay. Analyzing the social and economic problems discussed by Luther and his friends and followers, he concludes that some of their pamphlets did reveal a certain sympathy for the peasants. Hillerbrand then asks whether the articles advanced by the peasants during the uprisings of 1524 and 1525 revealed the influence of the reformers. Of 1,510 specific peasant articles analyzed only five per cent had to do with religion. Hillerbrand concludes that while the Reformation was one of several factors in the peasant uprising its impact was only very limited. I would like to add that the topic calls for analysis of more evidence than pamphlets and peasant articles. What, for example, was the religious situation of villages that rose up in revolt? What was the religious background of the peasant leaders? Above all, what role did the "Lutheran" ministers play in the uprising, men such as Lotzer, Strauss, or Hubmaier. Indeed, dozens of Lutheran-minded ministers were involved in the uprising. Obviously such a study might require archival work and concentration on a limited area.

In an essay also devoted to the peasant war Kyle C. Sessions analyzes a pamphlet written by Eberlin von Günzburg to the peasants of the Grafschaft Burgau in 1526 when new disturbances were feared. Sessions emphasizes the humanistic form and Lutheran theology of the pamphlet. He publishes its final, interesting section in which Eberlin relates his peace making efforts in the city of Erfurt during the uprising of 1525.

"The Two Social Strands in Italian Anabaptism, ca. 1525-1565," by G. H. Williams, introduces the reader to the rich work of Italian scholars. Following Aldo Stella, Williams argues that while the Italian-speaking Anabaptists of the south Tirol adhered to the theology, and particularly the Tridology and Christology, of the German and Swiss Anabaptists, the Valdesian-Anabaptist circles in Naples and later in Padua were influenced by Marranist and Philo-Judaic tendencies. According to Williams

it was due to Marranist influences that these Anabaptists eventually rejected the doctrines of the Trinity and completely humanized Jesus as the son of Joseph and a prophetic teacher—doctrines that were to lead to Unitarianism.

The intelligence and courage of women during the persecution of Protestants in England in the sixteenth century is the topic of Roland H. Bainton's essay, "John Foxe and the Ladies." Lengthy quotations from transcripts and letters taken from John Foxe's *Actes and Monuments* prove that these women argued very cleverly and defiantly during their interrogations and exhibited great fortitude during their imprisonment and execution. As always Bainton presents his subject in a persuasive form, but I find the tone of the essay a bit lachrymose.

Dannenfeldt's essay on "Wittenberg Botanists during the Sixteenth Century" offers a fresh picture of a little-known topic. It was partly due to Melancthon's open-mindedness and interest in science that Wittenberg played an important role in the early development of botany as a modern science. Numerous outstanding botanists either taught or studied at Wittenberg, among them the great Cordus, Belon, Cornarius, Lonitzer, Clusius, and Rauwolf. It was Cordus who initiated the use of botanical field trips. Dannenfeldt's learned little essay should encourage students to investigate the intellectual and scientific climate of other German universities in the sixteenth century. Lewis W. Spitz has contributed an essay on "Luther's Concern for His Students." Luther made great efforts to secure financial assistance and jobs for his students. Spitz also sheds new light on Luther's view of vocation and the Christian's place in society. Far from rigidly insisting that the Christian must stay in the position assigned to him, Luther thought that the choice of one's vocation depended on many factors, such as the needs of others, personal qualifications, preference, health, income, or living conditions. It was a happy idea to use Luther's correspondence to illustrate this point.

Gerhard Pfeiffer contributed an extraordinary essay entitled "Albrecht Dürer's 'Four Apostles': A Memorial Picture from the Reformation Era." On the basis of impressive literary evidence Pfeiffer maintains that the famous picture of the four apostles was painted after models, to be precise, after Melancthon. Joachim Camerarius, Hieronymus Paumgartner, and

Michael Rotting, all of whom were connected with the founding of Nuremberg's secondary school in May 1526. As these four men had done so much for the founding of the new school in the spirit of the four authors of the New Testament they were held worthy to appear as the apostles' representatives. It is an audacious thesis, presented with much learning. Carl C. Christensen has written a short but very instructive paper on "The Significance of the Epitaph Monument in Early Lutheran Ecclesiastical Art (ca. 1540–1600)." He discusses the religious subject matter of the paintings and relief sculpture and inscriptions against the background of Lutheran doctrine. The secular themes on epitaphs deal with the personal characteristics and elevated social position of the deceased. I hope Christensen will extend his studies also to Catholic epitaphs of which there is such an abundance in Germany.

Irmgard Höss's article on "The Lutheran Church of the Reformation; Problems of Its Formation and Organization in the Middle and North German Territories" sheds new light on the first and decisive steps in the establishment of church government in Ernestine Saxony, which secured the dominant role of the secular authorities in the government of the Church. Contrary to Electoral Saxony and Brunswick-Wolfenbüttel, with their consistorial form of church government, Prussia retained the office of bishop until 1584. But in Prussia, too, it was the secular government, not the bishops, that exercised the dominant power in the Church. Hans Liermann's essay on "Protestant Endowment Law in the Franconian Church Ordinances of the Sixteenth Century" discusses the stipulations for four types of pre-Reformation endowments with which the Lutheran governments had to deal: endowments for the benefit of the soul and motive masses, which were considered Catholic and eventually merged with the common chest; ecclesiastical endowments, which were preserved; benefice endowments, which sometimes were taken over by the secular authorities and used to pay salaries to the pastors; and hospital endowments, which were left untouched. The author emphasizes the care taken by the Lutheran governments to preserve the essential purpose of the endowments. Gerhard Hirschmann's essay deals with "The Second Nürnberg Church Visitation" held in 1560 and 1561, a few years after the devasta-

tions of the Second Margrave's War. Altogether fifty-four parishes were inspected. Half of the transcripts of the visitations are still extant. Hirschmann discusses the preparations for the visitation, the background of the visitors, their itinerary, and the results. While the professional standing of forty-six of the fifty-nine pastors examined was considered good or satisfactory many parishioners did not do at all well in the examinations. Hirschmann's essay is a valuable case study showing how much valuable information is contained in visitation transcripts.

In summary, this colorful and wide-ranging collection of essays proves that the social history of the Reformation is still a new field. Once the rich German archives have been fully exploited it may be possible to achieve a synthesis of the many powerful forces and tendencies of one of the most fruitful periods of European history.

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FRANCES A. YATES. *The Rosicrucian Enlightenment*. Boston: Routledge and Kegan Paul. 1972. Pp. xv, 269. \$15.00.

The Rosicrucian Enlightenment is the fourth provocative book from the pen of Dr. Frances A. Yates, whose name is just as distinguished in her native England as it is in this country. While a good deal has been written on the subject by enthusiastic, but not too well qualified authors, she has made it the concern of serious scholarship. The topic is intricate and does not lend itself easily to empirical and strictly objective treatment. Despite profound erudition and unparalleled acquaintance with European culture between the Renaissance and the so-called scientific revolution, the author often contents herself with genial assumptions and mere hypotheses. The Rosicrucian manifestoes that kindled imagination and stirred reverberating echoes appeared anonymously, and in some instances their origin and authorship were deliberately disguised to frustrate detection and identification.

Dr. Yates seeks a new approach to the complex field in that she links the emergence of German Rosicrucianism with the invigorating atmosphere that pervaded at least some parts

of the Empire after the marriage, in 1613, of Elizabeth Stuart to Frederick V of the Palatinate. This glorious period, extending for several years, had a rather distressing continuation—the transfer of the royal couple to Prague, the hopeless involvement in the Bohemian affairs, the flight to the Netherlands, and eventually the loss of the Palatinate. “It is not only possible but probable that the Rosicrucian movement, by the time it emerged into print, was connected with the Elector of the Palatinate”—thus the author sums up her findings (p. 54).

Dr. Yates has obviously gone to great labor to prepare herself for the writing of her book, which is distinguished by its breadth and wealth of details. But no matter how superb her narrative is, a critical reader cannot follow her line without pausing and raising challenging questions. The sketch of life at Heidelberg is concise and effective, but no perceptible link has been shown to exist between the splendid residence on the Neckar and the places from which the Rosicrucian manifestoes most likely emanated. Prince Christian of Anhalt, the chief architect of Frederick's foreign policy, stood high above not only his sovereign but also most other Protestant princes, yet his interest in subtle intellectual problems has not been established. Prague is a vague term, used to designate both the imperial court under Rudolph II, the Protestant majority of the Czech Estates, refugees from other countries, men of genius and charlatans. These things call for a more precise definition.

By her remarkably intelligent and inspiring work Dr. Yates has reopened a vast number of problems pertaining both to the early stage of Rosicrucianism and to its reconstruction after a temporary eclipse. The historians of ideas will be grateful for her successful effort to take the subject out of the range of occultist studies and make it a legitimate sector of intellectual history. Her illuminating book is likely to provide material for debate of its specific points for some time to come.

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PAUL FRITZ and DAVID WILLIAMS, editors. *The Triumph of Culture: 18th Century Perspectives*. (Publications of the McMaster University As-

sociation for 18th-Century Studies, volume 2.) Toronto: A. M. Hakkert. 1972. Pp. 387. \$12.00.

This volume is the second in the series issuing from the vigorous and fertile McMaster University Association for 18th-Century Studies, the first of which was *The Varied Pattern: Studies in the 18th Century* (1971), edited by Peter Hughes and David Williams. Both of these volumes emphasize the interdisciplinary approach and seek to explore the range and diversity of eighteenth-century culture. In the handsomely printed and well-edited book at hand the perspectives alluded to by the title are preponderantly historical ones, well worthy of the attention of the readers of a historical journal.

Social history is paid its due in several contributions. J. H. Plumb, in "The Public, Literature & the Arts in the 18th Century," discusses concretely the cultural revolution of the eighteenth century—"that process by which literature and the arts have ceased to be the pre-occupation of small specialised élites and have become available for the mass of society to enjoy." Complementing Plumb's article is one by Roy M. Wiles (to whom the whole volume is dedicated) that, with numerous specific illustrations, discusses "Provincial Culture in Early Georgian England." The essay by J. M. Beattie, "Towards a Study of Crime in 18th Century England: A Note on Indictments," is a sophisticated discussion of problems of methodology. Comparative social history is brought into play by George Rudé in his "Popular Protest in 18th Century Europe," a distillation of his important researches and one that discusses not only England and France but also the Pugachev revolt, the Lofthuus affair in Norway, and conditions in the Austrian dominions. Less persuasive, and close to being special pleading, is J. B. Owen's "Political Patronage in 18th Century England." The system, he contends, was no worse than what obtains in twentieth-century welfare states. "Bribery is now on a massive scale, and the muck is more evenly spread."

Economic history is represented by two articles on Spain. "The 18th Century Economic Analysis of the Decline of Spain," by C. Jago, emphasizes especially the ideas of Léon de Arroyal in the 1780s. Similarly, "Utility, Material Progress & Morality in 18th Century Spain," by W. J. Callahan, studies the conflict between

advocates of economic progress and clerical moralists. Finally, a very interesting and thoughtful essay combining local history with reflections upon the theory of history and the relationship of the historian to his subject matter is contributed by D. J. Russo in "The Deerfield Massacre of 1704 & Local Historical Writing in the United States."

The concept of culture is treated by the well-known art historian, Rémy G. Saisselin; the subtitle of his essay, "Tivoli Revisited or The Triumph of Culture," lends itself to be the title of the volume as a whole. Using such examples as Diderot's *Le Neveu de Rameau*, Mme de Staël's *Corinne*, and the David school of painting, Professor Saisselin concludes that "as concerns the eighteenth century one may consider culture [with its strongly historical, moral, civic, and intellectual bent] as the bourgeois invention *par excellence*." It is a complex and inviting argument, worthy of becoming a book.

The volume contains two articles on the history of ideas. A rigorous analysis by R. L. Walters of the various editions of Voltaire's *Eléments de la philosophie de Newton* ("Voltaire, Newton & the Reading Public") yields substantial and impressive results. The additions and successive revisions ended up by making the *Eléments* as much a metaphysical as a scientific work, which became an attack on Leibnitz and on atheism and expressed an increasing skepticism even toward science. L. Rosenfeld, in "Condillac's Influence on French Scientific Thought," with an admirably sinuous and supple style, summarizes Condillac's achievements in epistemology, psychology, and the theory of language.

The history of the theater is presented in an instructive essay by R. Morton entitled "'Blot and Insert Where You Please': The Fortunes of 18th Century Play Texts," which shows what companies of actors were likely to do to authors' completed texts. Two articles from the history of the arts are narrow in range but competent within their self-appointed limits. P. Walton writes of "The Educated Eye: Neo-Classical Drawing Masters & Their Methods"; H. Kalman discusses "The Architecture of Mercantilism: Commercial Buildings by George Dance the Younger [1741-1825]." Both essays are profusely illustrated.

Several of the articles in this volume reflect the current resurgence of interest in the theory and techniques of biography. This resurgence is partly in reaction to the more extreme exponents of the New Criticism who argued that biography was irrelevant to the understanding of a work of art. Partly it is because of the general acceptance of the concepts of identity and identity-crisis first revealed to us by the writings of Erik Erikson. Tracing a person's identity-crisis is intrinsically and inherently a biographical act, as illustrated in this volume by R. Van Dusen on "Goethe's *Dichtung und Wahrheit*: The Emergence of a Poet." Raymond Joly, in "La Fiction Autobiographique," shows what significant biographical information regarding an author can be gained from a novel written in the first person, for example *Gil Blas*. Indeed the difference between an avowed autobiography and such a novel is here affirmed to be slight. M. Joly's contribution is an example of the new and fresh kind of question that structuralists are beginning to ask about literary genres as well as a sensitive probing of the art of biography itself. An informative paper on "Archdeacon William Coxe as Political Biographer," "the first of the English political biographers to make extensive use of private manuscript collections," is contributed by Paul Fritz, one of the coeditors of the volume. "Boswell: The Cautious Empiricist," by C. Tracy, is a sparkling essay analyzing the reasons for Boswell's deficiencies "in a strabismic art such as that of biography." The essay by E. Cappadocia, "Benjamin Constant & Restoration Liberalism," reveals a man in whom "ambition and opportunism went side-by-side with a brilliant intellect and facile pen."

Of very great bibliographical as well as biographical importance is Ragnhild M. Hatton's "George I as an English & a European Figure." Professor Hatton's incomparable familiarity with the sparse and scattered source material, combined with her unrivaled knowledge of eighteenth-century diplomatic history, makes her essay a major event.

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GEORGE RUDÉ. *Europe in the Eighteenth Century: Aristocracy and the Bourgeois Challenge*. (History of Civilization.) New York: Praeger Publishers, 1972. Pp. ix, 290. \$15.00.

In a brief introduction to *Europe in the Eighteenth Century* George Rudé takes stock of the pitfalls besetting the historian of a complex period whose basic characteristics are implied in the title. He must contend in the first instance with the tendency to present prerevolutionary Europe as a sort of background to a revolution that was unique to France. The problem confronts him of how to stress movement and change, so pronounced after the 1760s, without neglecting the necessity to analyze existing structures and traditional continuities. There is the problem, too, of Britain, how to integrate trans-Channel developments with the course of events on the Continent.

To cope with those problems and tasks Rudé follows the approach of grouping them under three large sections. "People and Society," "Government and Ideology," and "Conflict." Within each of these sections he discusses in detail the themes relevant to it. Thus, in successive chapters he deals with these themes and their interrelations one by one: the population explosion, agrarian and industrial revolutions, the growth of a wealthy bourgeoisie, the Enlightenment and the enlightened despots, the *défi* to the monarchy and the aristocracy, popular protest, commercial expansion, and colonial wars. The procedure is not lacking in comprehensiveness.

Of the several chapters dealing with society the one on the aristocracy is a brilliant gloss on a major trend of the entire century. "In most countries in Europe," he writes, "an aristocracy of birth, wealth, and legal status was able to exercise a disproportionate influence over the lives of their fellow men, either as manorial lords, as monopolists of high office in army, church and state, or merely in their way of life or their opportunities for cultural attainment and foreign travel."

At the same time that the "privileged" were more than holding their own against the effort of government to make them contribute to the running of the state an increasingly opulent bourgeoisie was making extraordinary progress in the realm of the spirit. Rudé sees progress in the vitality of religious doctrine and practice as well as in new esthetic values and ideals. Continuing in that line of thought he discusses a new social climate that made it possible for ideas common to all philosophes to germinate, become adopted, and take root in France.

"Broadly . . . only those countries [meaning France] with a substantial middle class able and willing to adopt the ideas of the Enlightenment to its own use could really absorb them."

This analysis of structures and continuities is followed in the last of his three sections by a rounded account of the struggles of the several groups for control of the state within and warfare between the states without, all of which made the last decades what Rudé calls years of conflict.

One final problem remained for him to tackle directly, though he had already investigated it indirectly in preceding chapters—the problem of explaining why there was a revolution in France but not elsewhere on the Continent. On the score of France he states his position with exemplary clarity. "The French Revolution appears then," he concludes, "to have been the outcome of a combination of factors that arose from the conditions of the *ancien régime* . . . and in which all played their part."

As for the other states that escaped a revolutionary denouement, though much tension existed in one form or another, it was not the absence of any one or two particular facts but rather the absence of a combination of factors comparable to those in France that counted. Outside France there was no crisis of authority in society, Church, and state. There were no intransigent claims from the aristocracy, no propagation of radical ideas among wide sections of the people, no trigger of aristocratic revolt and popular rebellion. Outside France the old hierarchical order remained virtually unimpaired and dissatisfactions were absorbed in *ancien régimes* strikingly different from that in France.

With his strong narrative sweep, his skill in subordinating details to large developments, Rudé illuminates the obscurities inherent to his task with compelling persuasiveness. His endeavor is editorially aided by a workable index, carefully chosen illustrations, and a well-selected bibliography. It is patent throughout that by his major assumption concerning the process of history he subscribes to the orthodoxies of the Lefebvre school. But they are orthodoxies with reservations and qualifications that leave room for contemporary revisionist studies. He nowhere contends that the revolution had to take place as a culmination of a long evolution that made a capitalist bourgeoisie

master of the world. Nor are his assumptions synonymous with an objective law of the historical process: they allow for the play of the contingent and the unforeseen.

If both his generalizations and his details are open to disputation, no reader can easily deny the value of this substantial and engrossing work.

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CARLO BAUDI DI VESME. *Studi sul XVIII secolo: Le prime manifestazioni della rivoluzione d'occidente in Francia e nelle repubbliche oligarchiche (1748-1775)*. (Biblioteca di storia italiana recente, new series, volume 14.) Turin: Deputazione Subalpina di Storia Patria. 1972. Pp. 716. L. 15,000.

During the last few years Italian historians like Franco Venturi, Sergio Moravia, and Nicolao Merker have made noteworthy contributions to eighteenth-century studies. All three have concerned themselves primarily with the Enlightenment. Baudi di Vesme has chosen rather to examine the social, political, and economic aspects of European life from the 1740s through the 1770s. The book is divided into two parts: the first discusses France, and the second deals with the oligarchic republics of Genoa, Holland, and Geneva.

In these decades France faced a growing crisis both of leadership and internal stability. In contrast to the Prussian kings Louis XV failed to restructure and strengthen the state bureaucracy. He thus lacked a firm basis of power from which to deal with foreign and domestic affairs. Nonetheless, he continued to claim the prerogatives of an absolute ruler of a strong state. The result could only lead to disaster. By 1763 France's overseas empire had been lost; economic distress was widespread, and mounting criticism of the court and its policies discredited both the person and institution of the monarchy.

At the same time that Louis XV persisted in his ill-advised foreign policy the social character of France was being altered. A new commercial and manufacturing class was emerging, and it resented being excluded from government circles that remained in the firm control of the nobility and the great financiers. The increasingly acrimonious conflict between the king and the parlements and the criticisms

of the philosophes added to the general unrest and discontent. Baudi di Vesme agrees with Madelin that the first tocsin of revolution was sounded between 1740 and 1754.

The crisis within the small oligarchic republics of Genoa, Holland, and Geneva was no less serious. Each faced a different set of problems. What they shared was a superannuated political structure. After 1746 the Genoese oligarchy fronted increasing restiveness at home and in Corsica. It could rid itself of the Corsican problem by selling the island to France in 1768, but the unrest at home persisted to create growing tensions. In Holland a coup d'état in 1747 established William IV as hereditary stadholder, though Holland remained a republic in name. Conflict between the aristocracy and the supporters of the Orange family continued until the revolutionary changes at the end of the century. Geneva's problems derived mainly from the exclusivist character of the city's citizens who discriminated against the *natifs* and the *habitants*.

So much for the contents of this book. It is the result of wide-ranging research in Italian and foreign archives. The author reveals an encyclopedic knowledge of eighteenth-century sources, both published and unpublished. Unfortunately, the resulting book is pedestrian and disjointed. Except for the introduction and conclusion where some generalizations and comparisons are advanced each country is discussed as a self-contained unit. The parts remain parts and never become a whole. True, in his conclusion, Baudi di Vesme recognizes that the book is not an "organic whole," but he goes on to make sweeping generalizations that do not emerge from the contents of the book. Moreover, he claims to have proven that the Palmer thesis is untenable. Too many differences, according to Baudi di Vesme, exist between the American situation and the social, economic, and political accretions of the European past which created tensions that exploded in revolution. In addition, each revolution was the result of particular problems in each country. Thus, Baudi di Vesme rejects the idea that during the last decades of the eighteenth century Western civilization was going through a fundamental revolutionary change. He would have us concentrate on Europe exclusively, but he offers no persuasive reasons for so doing. In

conclusion, this is a disappointing book that, despite its impressive scholarship, falls short of its implied promise to deal with the "Western revolution."

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PEDRO SCHWARTZ. *The New Political Economy of J. S. Mill*. (London School of Economics and Political Science.) Durham, N.C.: Duke University Press. 1972. Pp. 341. \$11.75.

John Stuart Mill is one of the few major economists of whom an intellectual biography is both feasible and valuable. Professor Schwartz has admirably perceived and exploited this opportunity. He has given us a partial intellectual biography of Mill by tracing, through their various shifts and vicissitudes, his ideas about the "applications to social philosophy"—as Mill called them—of the *Principles of Political Economy*. Professor Schwartz summarizes his account under the four headings of "Trade Unions," "Laissez-faire," "Socialism," and "The Future of Society." Some of this is well-trodden ground—for example, the shifts of emphasis to and fro on the subject of socialism. But Professor Schwartz has much that is fresh and significant to offer. Nevertheless, in referring, on the subject of socialism, to Mill's "inexcusable subservience to his wife's opinions" (p. 191) Professor Schwartz rather abruptly dismisses, or fails to take into account, the arguments of H. O. Pappe on "the Harriet Taylor Myth."

The explanation of the adjective "new" in his title seems to be that Professor Schwartz wishes—up to a point quite justifiably—to emphasize the originality of Mill's discussion, especially in books 4 and 5 of his *Principles*. Yet Professor Schwartz himself emphasizes how "in practically every period in the history of economic doctrines, economists have thought that they were witnessing the birth of a 'new political economy'" (p. 15). Indeed, Professor Schwartz also emphasizes how, in some ways, Mill's *Principles of Political Economy* were not nearly "new" enough. He refers to Mill's "rigidity in his unwavering acceptance of the main tenets of Ricardian . . . doctrine" (p. 49). The point is that one cannot separate so clearly his "novel" social applications from his rigid ac-

ceptance of the "older" theories. Mill's social philosophizing was inevitably rendered somewhat insubstantial and hypothetical so long as he held to such central Ricardian positive theories as the "law" of population and natural wages, and the "law" of markets or the Smithian saving-is-investing doctrine. Though Mill made some moves (in book 4, chapter 5 of *The Principles*) toward jettisoning the latter doctrine as one of the now obsolete teachings of the old political economy, he never broke out explicitly. It was only possible for Mill to point toward new vistas that mankind might (or might not) be able to approach in the future. He could not advance himself, encumbered as he was by the Ricardian theories that fixed such severe limits for wages and public spending. Still, though he may overemphasize their novelty, or rather the significance of their novelty, Professor Schwartz has written an important and deeply interesting book about Mill's "applications to social philosophy" of his essentially Ricardian economic theories.

T. W. HUTCHISON
University of Birmingham

was let slip, and, although Newgate was rebuilt, an observer noted seventy years later that it "continues down to our day one of the worst hot-beds of vice and moral disease in London." Meanwhile, some of the reformers—neither John Howard nor Elizabeth Fry were among them—had had their way, and the 1840s were marked by the experiment of the "separate system," which pushed the rate of insanity in the new model penitentiary at Pentonville up to ten times the rate in other prisons in Britain.

This is the story of crimes perpetrated against criminals and other prison inmates rather than of crimes committed by the criminals themselves, and Mr. Babington, as James Callaghan writes in the preface, has done a useful service in writing it. Yet crime and retribution (including a vicious prison system) are social phenomena and operate in a social context. But this is a facet that Mr. Babington has not explored, and it is a weakness of the book. This is an excellent compendium of knowledge, but it is not a social history of criminals and prisons.

GEORGE RUDÉ
Sir George Williams University

ANTHONY BABINGTON. *The English Bastille: A History of Newgate Gaol and Prison Conditions in England, 1188–1902*. New York: St. Martin's Press. 1972. Pp. 250. \$7.95.

This is the story of prison life in Newgate (and a few other prisons) over a period of seven hundred years. It is an informative and well-written account, but it makes depressing reading with its almost unchanging picture of filth and squalor, cruelty, avarice, and official indifference and its long succession of villainous keepers and turnkeys. Yet there are some bright patches to relieve it, such as the exploits of the elusive Jack Sheppard and the unusual humanity of one keeper, Richard Akerman. Such patches, however, are few. One of the most depressing features of the whole story is the stolid resistance offered by the authorities and the public to any plans for reform. A golden opportunity presented itself in 1780 when, during the Gordon riots, the "lower order" of Londoners performed the signal service of razing the prison to the ground. (This was three years after John Howard's first indictment in his *State of the Prisons*.) But the opportunity

S. B. CHRIMES. *Henry VII*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xv, 373. \$16.00.

"Schooled in adversity and disappointment," Professor Chrimes relates, the twenty-eight-year-old Henry Tudor ascended the English throne. He founded a dynasty that prevailed against pretenders, heretics, and foreigners. Sometimes seen as a more mysterious character than his son, grandson, or two granddaughters, Henry has not inspired the romantic interest or partisanship that a Henry VIII, Edward VI, Mary, or Elizabeth I have. Even staunch defenders of the Yorkist Richard III wish to absolve Richard of his nephews' deaths rather than to discover how Henry, whom they indict, did it. Ironically, they view Henry as so uninteresting a murderer that neither his personality nor crime merit investigation.

While Chrimes concludes that Henry was no murderer—the author believes Richard was responsible—he is not really interested in plucking out the heart of Henry's mystery. Admittedly the surviving voluminous records are not of the sort that facilitate a Freudian or

Eriksonian biography. Again, Chrimes's *métier* is political-administrative-constitutional history. For example, he has written two volumes essential for introducing students to fifteenth-century England, *An Introduction to the Administrative History of Mediaeval England* (1966) and *Lancastrians, Yorkists and Henry VII* (1966). Legal historians are also indebted to him for his exemplary edition of Sir John Fortescue, *De Laudibus Legum Anglie* (1942) and his revision of Sir William S. Holdsworth, *History of English Law* (1956). The virtues of these works are readily apparent in his *Henry VII*: patient historical reconstruction, close attention to detail, plausible explanation, and lucid presentation.

Chrimes's contribution is, as he says, an "interim report." He bases his study on a thorough review and analysis of all the printed sources as well as a discriminating reading of all secondary works. His synthesis of existing views of Henry's political-constitutional-administrative-fiscal ambience is a major contribution to Tudor historiography. Like the authors of two companion volumes in the English Monarch series—J. J. Scarisbrick, *Henry VIII* (1967) and Frank Barlow, *Edward the Confessor* (1970)—Chrimes is a demythologizer. His Henry VII is not a brave new monarch leading the English into the modern era. The old legend of Henry's avarice, however, is given some credence. Henry's attitudes are set in the context of his situation as a personal monarch ruling in the days of highly personal monarchy. Chrimes does not inflate Henry's importance by making him a creative systematizer, innovator, or enforcer of the law, nor a brilliant administrator or paternalistic despot. Thus Henry becomes the heir of his immediate predecessors and the product of a long, medieval tradition; hence Chrimes gives the final *coup de grâce* to the notion of Henry's "new monarchy." Chrimes concludes that Henry was "not a creator but rather a stabilizer" and, as such, an important link in the development of English institutions.

Chrimes utilizes the work of such scholars as G. R. Elton, Paul Murray Kendall, J. R. Lander, W. C. Richardson, R. L. Storey, and B. P. Wolfe. However, he does not accept their work without reservation and in almost every case corrects errors of fact or interpretation. The result is a solid study worthy of its

series. Nonetheless, Chrimes's observation that Henry VII was "not personally interested in religion" is jarring, and one wonders why Henry would have requested ten thousand masses in his will if he were not. Because Chrimes informs us that Henry did not have an illegitimate son nor an army of paid informers and never fined the earl of Oxford £10,000 for illegally retaining, as Bacon maintained, the bloom is off Henry's new monarchy. Thus, Tudor historians must revise their lectures. Given the nature of the evidence, perhaps we will never have the intimate biography that we want.

M. J. TUCKER

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Buffalo

ARTHUR J. SLAVIN, editor. *Tudor Men and Institutions: Studies in English Law and Government*. Baton Rouge: Louisiana State University Press. 1972. Pp. viii, 294. \$11.95.

Dedicated to Professor Walter C. Richardson, this volume of essays by his friends and students provides new light on several corners of Tudor administration. The essays, mostly modest in length and unpretentious in scope, generally reflect the same careful scholarship and the same concern with the governmental machine in operation that characterized Professor Richardson's own major works.

The theme that unites most of the essays is concern for power—how Tudor ministers acquired it, how they employed it, and particularly the restraints that served to check it. These writers tend to find that Tudor officers were repeatedly frustrated by intramural conflicts and jurisdictional disputes, by restrictive administrative traditions, and by the stubbornly maintained interests of other functionaries. That is especially true of the longest of the essays, W. J. Jones's examination of the Exchequer Court of the County Palatine of Chester. Even though the chancellors were nearly always major court personalities, they were never able to overcome problems caused by squabbles with the city of Chester, factiousness of underlings, jurisdictional limitations, and the impossibility of close supervision from court. Indeed, some of the influence the Chester Exchequer Court retained simply reflected the

power of figures like Leicester and Egerton who were chancellors. Jay P. Anglin, looking at the efforts of the church hierarchy to enforce discipline on the Puritan clergy of Essex through the "Bawdy Courts," comes to similar conclusions. Because the courts had been given new duties, because of their dependency on reluctant and ill-prepared church wardens, because of opportunities for delay and evasion inherent in the canon law procedure, and because of the autonomy of the lower judges between visitations, the courts had no incentive for bearing down hard on the "godly" clergy. R. W. Heintze finds that the Statute of Proclamations of 1539 was ineffective because of the limits placed on the time for prosecution and on quorum requirements for the court when cases were heard. A. J. Slavin draws attention to the rivalries between major offices that killed reform of Chancery in the last years of Henry VIII. DeLloyd J. Guth concludes that the Court of Exchequer Pleas, once a major organ for protecting the interests of Crown officers, had declined into insignificance, a place where local officers could be sued for debts.

Only one of the articles, that of Mortimer J. Levine on Henry VIII's prosecution of the duke of Buckingham, stresses the breadth of Tudor power rather than its limitations. Levine sees the judicially and morally indefensible treason charge as a kind of flexing of the royal muscle, a demonstration to the realm that Henry could destroy any subject, no matter how great or how near the throne. Stanford Lehmberg purports to deal with Sir Thomas Audley's use of power, to see if his soul were actually as black and his heart as hard as the marble of his tomb, as his epitaph proclaims, but he is actually more interested in Audley's accumulations of offices and lands. The subject matter of two of the essays does not fit into the structure that has been imposed: J. R. Lander's on the 1475 campaign of Edward IV, which concludes that the French wars could no longer generate enthusiasm and had to be justified to the country in terms of defense; and Louis Knafla's study of the Elizabethan Inns of Court, which suggests a "revolutionary" expansion in matriculations and transformation in content of lectures.

The final essay, by G. R. Elton, is set apart in a different way: it is not a result of new research, as the others are, but a new round in

his debate with Joel Hurstfield on law and liberty in the Tudor state. Elton reaffirms here his conviction that Tudor rule was moderate, limited, and essentially constitutional. His emphasis is again on law: the prerogative law was considered a department of the common law; the Tudor monarchs did not make law by themselves or wish to acquire the power to do so and did not transgress the conventions of law in their rule. Therefore Tudor rule was not despotic. Hurstfield, on the other hand, is concerned not with law as such but with the area of freedom in which the subject feels able to move. For him despotism is a government which "enforces its will, suppresses dissent, and rules a society whose members have few means of influencing major decisions." By these standards, which Elton does not really refute, the Tudors were despots. Their perspectives are so different—Elton's legal, Hurstfield's humane—that they cannot meet on the same grounds. Each of these eminent scholars, ironically, has been forced to the conclusion that the other does not understand the essential nature of the Tudor state. Out of this kind of disagreement important new interpretations may come.

ROBERT W. KENNY

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BERYL PLATTS. *A History of Greenwich*. Newton Abbot: David and Charles. 1973. Pp. 231. £3.50.

A royal residence from the fifteenth to the seventeenth centuries and an important port in the days of sail, Greenwich, four and a half miles downstream from London Bridge, always had its fortunes joined with those of the metropolis. By Victorian times the two were physically linked: "London has stretched out its arms to its pretty cousin," observed George Rose Emerson in 1862; "fourpenny steamboats and crashing railway trains have cemented the union . . . and Greenwich . . . is really now nearly as much a suburb of the great city as Hackney." Greenwich deserves a full and careful modern study as much as any London suburb. Mrs. Platts's book unfortunately maintains the grand tradition of English local history in its anecdotal approach and its tendency to devote attention to the periods of Greenwich's past in inverse proportion to the amount of hard evidence available. Time spent demon-

strating its possible identity with pre-Roman Trinovantum and connecting it to incidents related by Geoffrey of Monmouth would have been better devoted to expanding its well-documented history from the Tudor period to the present. The book has a brief bibliography but no footnotes; the pleasant illustrations include a portion of Rocque's *Survey* of 1746 but no other maps.

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Vassar College

JOHN CANNON. *Parliamentary Reform, 1640-1832*. New York: Cambridge University Press. 1973. Pp. xiv, 333. Cloth \$17.50, paper \$6.95.

This book is a useful pioneer presentation of the story of parliamentary reform in Great Britain from the English Civil War to the Reform Act of 1832. To avoid discursiveness Dr. Cannon has narrowed his investigation "to the two most important aspects of reform—namely changes in the franchise and changes in the distribution of seats." Other proposed reforms receive mention only in relation to these themes. His first chapter and supporting appendixes bring out interesting parallels between ideas and programs of the mid-seventeenth and the early nineteenth centuries. An extensive range of recent publications and original research is made the basis for rejection of an overstatic picture of eighteenth-century political society, which the work of Namier may have helped to consolidate. We may note here, however, the value of Namier's last and greatest scholarly enterprise, *The History of Parliament*. Five volumes now published covering 1714-90 illuminate the gradual transformation of the political system (which, as Mr. Cannon says, changed "subtly but inexorably, decade by decade") and so contribute to a better perspective of later eighteenth-century reform movements.

Cannon's book contains much interesting matter on the events leading up to 1832. Also the author has fresh things to say about the Reform Act; in particular, he emphasizes its conservative character and intention. He avows a special interest in tracing the relationship between ideas and actions, "to see why arguments which are pressed ineffectually at one moment become politically viable the next." In specific contexts this is achieved. But one would

have welcomed some reflection on the more general change of intellectual climate that was gathering momentum between the mid-eighteenth century and 1832. Men had previously thought in terms of a static order. Thus the science of politics was conceived as already established by human experience for all time, equally valid in ancient Athens and modern Britain. After 1750 growing knowledge and intellectual speculation were undermining this world picture at many different levels. By the early nineteenth century such thought patterns were yielding to ideas of flux, change, and human capacity for deliberate, engineered improvement. No doubt Mr. Cannon is right in saying that Bentham had little influence on the Whig reformers, owing to his tortuous style. But the advancing revolution in ideas and attitudes, of which Bentham's work was only a part, helped to sap Burkeian faith in prescriptive authority and to generate and sustain the public pressure before which Earl Grey and his colleagues felt compelled to yield.

IAN R. CHRISTIE
University College London

JUDITH BLOW WILLIAMS. *British Commercial Policy and Trade Expansion, 1750-1850*. With a bibliographical chapter by DAVID M. WILLIAMS. New York: Oxford University Press. 1972. Pp. x, 514. \$24.00.

This book appears in unusual circumstances. It was newly complete at the time of the author's death in 1956 but has taken sixteen years to achieve publication. Its appearance now is fully justified, for it is a work of fine scholarship on an important subject. Probably no other single volume has presented so much new information about the detailed content and conduct of British commercial policy. Judith Williams had not only thoroughly sifted the voluminous official papers but had mastered a larger and more varied range of contemporary printed materials than any other writer in the field. She never allowed the resulting abundance of material to overwhelm her, but presented it with great clarity.

Commercial diplomacy is the aspect of policy that receives most attention. Thus the book is very different from most studies of Britain's approach to free trade, which it both supplements and corrects. Free trade policies were

not achieved solely through the shift in the balance of power between rival interest groups at home. Their content was not molded simply by the conversion of politicians to a belief in minimal regulation of trade and by a consequent unilateral decision to legislate accordingly. This is a book about governments that were constantly active: always informing themselves about the policies of other states and adjusting to them; gaining more security for British merchants first in one place, then another; giving a little here to take a little there; resisting the more stifling foreign restrictions without demanding concessions on a scale that was politically impracticable. It is not the whole story, but it was a missing piece and is missing no longer.

The book's contribution to the history of trade expansion is much weaker. The main subjects studied under this heading are the acquisition and development of colonies and trading stations and the revelation of new territories by journeys of exploration. Much of the information is fascinating, but it throws little light on the growth of trade in most of this period. The approach results in inadequate attention to Continental Europe, which was still Britain's largest export region and had great potential for commercial expansion. This shortcoming of the book is the more serious because the author was remarkably incurious about the economic effects of particular measures of commercial diplomacy. She cited occasional figures for the value of trade with a few countries in particular years, but had no feeling for the long-term movement of quantities. When a set of duties or prohibitions was changed, or a new commercial treaty was brought into effect, she made little enquiry about the nature and magnitude of changes in the related trades. Only in a few cases where the changes were apparently negligible does she show a realization that trade expansion might depend far more on such things as transport and the nature of local industry than on commercial diplomacy. In this respect the book suffers from an old-fashioned treatment of its subject; but its study of the details of commercial diplomacy has old-fashioned virtues for which it will long be used.

WILLIAM ASHWORTH
University of Bristol

PATRICIA B. CRADDOCK, editor. *The English Essays of Edward Gibbon*. New York: Oxford University Press. 1972. Pp. xix, 650. \$33.75.

Editing Gibbon is no easy task. His handwriting is firm and clear, but the vast range of his interests and his enormous erudition require of the editor a mastery of recondite references and a hard-won intimacy with one of the finest historical minds we have ever known. Fortunately, this edition is the product of this type of devotion and scholarship. With the exception of the autobiographical works—memoirs, journals, and letters—and the *Decline and Fall* it contains all of Gibbon's writings in English, from his bold chronology of world events, written in 1751, to his final reflections on the vocation of history, produced in the last months of his life. As a result of Patricia B. Craddock's labors we may glimpse Gibbon at work over forty years of his active intellectual life.

Gibbon's friend and literary executor, Lord Sheffield, created particular problems for all subsequent editors of the historian's memoirs and journals, for he believed it necessary to purge these papers of much important information. The editor of the present edition faced similar problems and overcame them. Where Sheffield corrected Gibbon's spelling we now read the original and have a footnote with the Sheffield variation. Hence, we have essays and notes, marginalia and commonplace books, just as Gibbon left them and not as his friend thought we ought to see them. Craddock's restoration satisfies our wish to read Gibbon in the original, and it is, moreover, interesting from a graphic point of view: uneven punctuation and misspellings make many of these pages curiously pleasurable to the eye. We have the authority of the original manuscript clearly deciphered for us, and we can still see what the manuscripts look like.

The plan of the edition is chronological, and we detect the turning point in the development of the historian to be the publication of the *Decline and Fall*. Before the *Decline and Fall* years he roams through chronologies and notes, commenting freely and critically on popes and ancient kings, on the antipathy between philosophy and Christianity, and on the history of the laws of England. After his great work reaches the public he is concerned with

the craft of history in a professional way. The longest, certainly the most entertaining, piece in this volume is Gibbon's *Vindication* of his fifteenth and sixteenth chapters. This brilliant bit of revenge was, of course, printed during Gibbon's lifetime, but the editor has done well to reprint it here. The *Vindication* forms a significant part of Gibbon's self-awareness as a historian, and it should be read in combination with his other notes regarding possible "corrections and improvements" for the *Decline and Fall*. Gibbon condescended to answer Henry Edward Davis's fatuous diatribe because he wanted the *Decline and Fall* to be accepted as the work of an accurate and critical historian, not merely as "a tale amusing enough." The same concern for critical history permeates his last works, notably his lengthy *Antiquities of the House of Brunswick* (1791), which until now has been available only as a fragment of the original, and his "Address," which he wrote only months before his death. In this last piece Gibbon calls for the publication in England of "our latin memorials of the middle age." He cites the works of the Benedictines as the model for this type of scholarly activity. He knew to whom he owed his debts.

More might be asked of the editor. There could be additional introductory material, and it would be convenient if the dates of composition were placed nearer the essay or marginalia in question; but it seems more appropriate to express gratitude for the accomplishments of this handsome edition. Obscurity and Lord Sheffield no longer separate Gibbon's audience from the complete body of his English works.

VICTOR G. WEXLER
University of Maryland,
Baltimore

BERTRAM H. DAVIS. *A Proof of Eminence: The Life of Sir John Hawkins*. Bloomington: Indiana University Press. 1973. Pp. xii, 436. \$10.00.

Although Dr. Johnson's unclubbable friend John Hawkins deserved his comment that "it is no less a proof of eminence to have many enemies than friends," Mr. Davis documents more acceptable achievements in a surprising variety of public roles. The only son of a carpenter, Hawkins briefly studied architecture,

became an attorney, acquired brilliant associates and aristocratic neighbors, and made his mark as antiquary, editor, music scholar, pamphleteer on local issues, magistrate, and biographer. As we can infer from this *Life* if we wish—Mr. Davis risks few generalizations—Hawkins's career demonstrates what was open to a diligent, energetic, and intelligent bourgeois of the eighteenth century, particularly if he married an heiress.

Limiting himself largely to what can be documented (unlike Percy Scholes in his 1953 *Life of Hawkins*), Mr. Davis concentrates on the external and the public. But if treating biography as a series of research reports (for example, "Chapter 6, A Shakespeare Quartet," on four scattered contributions) avoids Hawkins as a person, it lucidly shows us a great deal about his activities, particularly as magistrate. From freshly examined sessions papers, Mr. Davis usefully describes the operation of the quarter sessions and general sessions in the county of Middlesex; the problems of keeping the members of the court honest; day-by-day courtroom conditions; an intricate bureaucratic squabble over an official's incompetence; the details of prison maintenance and administration; and the building of a new sessions house at Clerkenwell Green, a major achievement of Hawkins's administration. Re-examining all the old evidence and uncovering a good bit that is new, Mr. Davis is able to exculpate Hawkins from M. Dorothy George's charges of dereliction of duty in the Gordon riots and cruelty toward the accused in his court ("Sir John Hawkins as a Justice of the Peace," *National Review*, 88 [1926]: 433-41).

On Sir John as a literary figure, Mr. Davis can add a degree of precision to the listing of early contributions to the *Gentleman's Magazine*, to pamphlet attributions, and to the quarrels deriving from Hawkins's literary competitions with Moses Browne over editions of the *Compleat Angler*, with Charles Burney over histories of music, and with Boswell and Mrs. Piozzi over biographical works on Johnson. But he will not speculate why Hawkins—evidently decent, able, and learned—insulted Burke beyond the Club's tolerance; antagonized old friends like Garrick and Percy and repelled likely new ones like Bentham; and made a point of attacking the best music and literature of

his day. Rather than hazard a possibly arbitrary definition of Hawkins the man, Mr. Davis provides scholarly assessments of the surface of his life. If Hawkins was only an attendant on literary history—Boswell's Rosencrantz or Guildenstern—who happened also to be very busy when off stage, this would be enough. In persuading us to the contrary, however, Mr. Davis creates a need he does not satisfy.

MORRIS GOLDEN

University of Massachusetts,
Amherst

JOHN W. OSBORNE. *John Cartwright*. (Conference on British Studies Biographical Series.) New York: Cambridge University Press, 1972. Pp. ix, 173. \$12.95.

Professor Osborne has written the first serious biography of Major John Cartwright, "the Father of Reform" (1740–1824). The weaknesses of Osborne's book derive from the limited available sources. Cartwright's doting niece published an uncritical *Life and Letters* (1826) just after his death, and those volumes still provide most of the personal details about the octogenarian radical. Lacking family or personal papers, students must depend on the major's voluminous and rather dreary political writings, some correspondence in other collections, and the often unflattering comments of contemporaries. Professor Osborne has drawn together what little we can readily know about John Cartwright, the person, but for all of Osborne's sympathy the man remains less than life size. He seems a crashing bore. Cartwright was frail, socially insensitive, didactic, stubborn, narrow, and brave. The major moved with a strong sense of family loyalty. He was friendly with such diverse, difficult people as Earl Stanhope, an irascible genius given to disinheriting his children, or Thomas Wooler, the colorful, quarrelsome, radical publicist.

Cartwright had a good eighteenth-century democrat's disdain for the lower orders, the unruly poor, the "crowd." Osborne repeatedly states that Cartwright was economically naïve and thus insensitive—for instance, to the basis of Luddite discontent. But Osborne's own evidence runs precisely to the contrary. Cartwright shifted from a marginally profitable estate to a substantial holding at Brothertoft, near Boston.

Lincolnshire. With an eye to the rapid growth of the cloth industry, he organized a cartel of woad growers to hold down wages and control supply. He derived immense profits from the rigged market in this vital dyestuff. Cartwright's brother Edmund invented the power loom and went bankrupt. The major fought the patent infringements in the courts and helped to secure the £10,000 award to his brother in 1809 from Parliament. While unhappy that his nephew Edmund would not continue the profitable exploitation of Brothertoft after he died, Major Cartwright recommended banking to the lad as the easiest quick route to wealth.

Osborne is principally concerned with the political Major Cartwright. He leads the reader carefully and perceptively through the repetitive, limited range of Cartwright's ideas. Osborne demonstrates that there was some slight growth, although the grounds laid out in *Take Your Choice!* (1776) were modified more in detail than substance. Cartwright's program—universal manhood suffrage, vote by ballot, equal electoral districts, single member constituencies, annual parliaments—would be enshrined in the Chartist platform and, with the exception of annual parliaments, was to be enacted in the course of the next century. His notions on jury selection would shortly be accepted, while his advocacy of a freeholder militia added a quaint, outdated quality in the era of the French and Napoleonic wars.

How is one to explain the importance and influence of an incompetent practical politician, a less-than-marginally-successful political agitator, a man of relatively few novel or striking ideas and little literary felicity? Most of his notions were advanced by other contemporaries. Thomas Paine said more better but had less lasting influence in England. For one thing, as Osborne points out, Cartwright had longevity and consistency on his side. He earned the sobriquet "Father of Reform" by persistence and seniority. But he also won it by establishing his program on the only basis for successful radical propositions in England. Everything he sought, Cartwright argued, was merely a restoration of lost rights and freedom. The major was completely English. Scotland and Ireland barely came within his purview. He was pro-American, not anti-English, during the American Revolution. While a reformer, he was not a revolu-

tionary. He never succumbed to radical pro-French hysteria. He had the strength and limitations of his class, his age, and his patriotism. Professor Osborne and the Conference on British Studies Biographical Series have served us well with this volume.

EUGENE C. BLACK
Brandeis University

RICHARD W. DAVIS. *Political Change and Continuity, 1760-1885: A Buckinghamshire Study*. (Library of Politics and Society.) [Hamden, Conn.:] Archon Books. 1972. Pp. 262. \$11.00.

This is a lively and contentious book of wider significance than its title may suggest. Professor Davis is concerned to challenge the view that the changes introduced into the British electoral system by the 1832 Reform Act were of only marginal importance and that the landed class was able to retain the greater part of its power because of the continuation of the habit of deferential voting in the counties and many of the smaller boroughs. He does not, of course, deny that the personnel of Parliament remained overwhelmingly landed after 1832, but he insists that this was not primarily because rural voters were dragooned into supporting them. Instead, he stresses the independence of many electors, the interest shown in public issues, the swings of opinion hardly accounted for by swings of property, and the skill and flexibility employed by the leaders of county opinion in order to maintain their position.

Professor Davis approaches his task with the zeal of an inquisitor rooting out heresy. His first victim is D. C. Moore, who has always seemed the most vulnerable of the exponents of "continuity." The attack on Norman Gash's arguments is, perhaps, less successful. Indeed, I am not sure that Davis and Gash are as far apart in their conclusions as the former appears to believe. The wealth of illustration that Professor Gash offered in support of his contention that many pre-Reform practices died hard may have obscured the moderation of his final assessment: while pointing out, for example, that a number of proprietary boroughs survived 1832, he admitted that "naturally the great era of the borough-proprietor had passed away for ever." Nor does Davis deny that landlords after 1832 tried to influence voters and that some

continued to evict recalcitrants. He is certainly able to demonstrate that Buckingham changed from a two-seater borough with no questions asked to one in which the duke could not be sure of keeping out an opponent—but Gash claimed no more than one seat for the Grenvilles after 1832.

What Professor Davis does establish is that there were no constituencies in Buckinghamshire after the Reform Act in which one could say of the landed class—in Professor Moore's words—that its command was absolute. The argument would carry even more weight were the book not marred by misprints and misdatings and cumbered with an erratic index.

Professor Davis is well aware that it would be rash to generalize from the experience of one county. It is unfortunate that so much of his evidence comes from Aylesbury, which, even in the snug days of the Pelhams, had a reputation for factious independence. It is still difficult to form an opinion whether the absence of domination in Buckinghamshire reflects merely the peculiar configuration of the landed property there or a growing independence on the part of voters generally. Nevertheless, Professor Davis has reopened an important discussion and produced a Bucks to set against Professor Gash's Berks. To what extent Victorian voters were deferential should prove sufficiently controversial to keep a whole generation of historians hard at work.

JOHN CANNON
University of Bristol

W. R. WARD. *Religion and Society in England, 1790-1850*. New York: Schocken Books. 1973. Pp. ix, 339. \$10.00.

The Early Correspondence of Jabez Bunting, 1820-1829. Edited for the Royal Historical Society by W. R. WARD. (Camden Fourth Series, volume 11.) London: the Society. 1972. Pp. 240. £3.00.

Victorian religion has recently attracted considerable attention from sociologists and historians, but scholars of both persuasions have tended to study Victorian religious institutions from the inside. They have written ecclesiastical histories and sociologies of sects rather than studies that probe seriously the complex links between religion and society. The social history of religion in nineteenth-century England has

suffered from the absence of general ideas; surely it is because it inhabits such a sparsely stocked field that the well-known Halévy thesis remains so well known.

In a book bristling with sharply etched ideas about many aspects of early nineteenth-century religion, W. R. Ward, professor of modern history at Durham and current president of the Ecclesiastical History Society, has developed a general argument that has considerable interpretative power. Ward concentrates his attention, and much of his research, on Wesleyan Methodism, especially Methodism in the industrial districts of Lancashire and the West Riding. It is important to bear in mind the Methodist perspective of his work, for it influences profoundly the general framework he constructs for understanding the social history of religion in the first half of the nineteenth century.

Ward argues that at the end of the eighteenth century the vital center of English religion was located in the Evangelical revival, which he considers the religion of the people. The revival was nondenominational and nondoctrinaire, fostering nondenominational institutions and pragmatic theology. The revival was also socially open, appealing to men and women from all levels of society. During the first two decades of the nineteenth century, especially during the troubled years from Luddism to Peterloo, social tension generated pressures that encouraged movement away from the openness of the revival toward more rigidly defined and highly organized denominations. Ward's intricate description of this process at work among the Methodists is perhaps the most impressive portion of this generally impressive book. He shows how economic dislocation created a rift between the radical poor and the rich within the society and how deflation forced the itinerant ministers to become allies of the wealthy and, consciously in some cases, agents of social control.

During the twenties denominational lines continued to harden under the continuing influence of a complex interrelation of social and institutional pressures. By the thirties, the vitality of the revival had been destroyed in the name of denominational discipline. Denominations, moreover, had become aligned with social classes, and the poor, on the whole,

were left out. Too narrowly constructed to contain the energy of the revival or to comprehend major social tension, the denominations, in the age of the Chartists, carried religion away from the mainstream of national life.

In this provocative study of the social sources of denominationalism, written from assumptions strikingly akin to Richard Niebuhr's, Ward has left many questions unanswered. Is it fair to write off the Established Church as a victim of the 1790s? Is not the battle between Church and Dissent in the thirties and forties a sign of vitality, even if the working classes were not much involved? Most important, do denominations actually become closely aligned with social classes? This proposition, central to Ward's argument, is not convincingly demonstrated in this book. It is to be hoped that Ward's work will stimulate more investigation of the problems he has illuminated and that the clearheadedness of his arguments will be an example to those who follow.

A repository that researchers might well make more use of is the Methodist Church Archives, the location of the Bunting correspondence and much else as well. The enormous Bunting correspondence, as Ward points out in his useful introduction, is the major manuscript source for the history of Wesleyan Methodism for the thirty years after 1820. Ward's well-edited selection of the correspondence includes all the letters of importance written between 1820 and 1830. Bunting's son published the important letters for the period before 1820 as part of his father's biography, and Ward hopes in the future to publish a selection from the period of Bunting's greatest influence, the two decades after 1830. Bunting's correspondence is peculiarly interesting because it contains a very large number of "in" letters from preachers scattered over England. It is the authoritative record of Bunting's activities at the center of power, and an important source of information about Methodism in the field as well.

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LEONARD G. WILSON. *Charles Lyell. The Years to 1841: The Revolution in Geology*. New Haven: Yale University Press. 1972. Pp. xiii, 553. \$17.50.

Britain produced a remarkable crop of distinguished nineteenth-century geologists, most

of whom became the subject of monumental Victorian biographies. Fascinating though these works are, they hardly satisfy the modern historian, and Professor Wilson's study of Sir Charles Lyell has been awaited eagerly as a step toward a better understanding of the men who did so much to create modern geology. Sadly, the book is a disappointment.

The volume traces Lyell's life down to the eve of his first visit to America, and by availing of a wealth of hitherto unused manuscript material Wilson is able to depict Lyell's activities in considerable detail. Indeed in many places a pruning knife might perhaps have been employed upon the minutiae to good effect. In other places (for example, pp. 96–103 and 173–77) there are overlong digressions possessed of little relation to the main theme, and throughout the volume the informed reader will be repeatedly aggravated by minor errors. The year of James Hutton's death is incorrectly quoted (fig. 8), for example, there are textual contradictions on pages 5 and 11, no British body has ever been styled "the Ordinance Survey" (p. 424), and the Saxon town of Freiberg is invariably misspelled as "Freiburg." A number of important recent works are strangely absent from the bibliography (among them Wilson's own 1969 essay on Lyell's *Principles*), and it is odd that a study which aspires to be definitive should contain no reference to the earlier Lyell biographies by T. G. Bonney and Sir Edward Bailey. Even the book's journal-like comprehensiveness may in part be illusory because at least one important event in Lyell's life has virtually escaped Wilson's notice—his 1818 visit to the scene of the catastrophic debacle in the Val de Bagnes.

Annoying though they are, such defects might be forgiven had Wilson offered a penetrating insight into Lyell's accomplishments, but this he has totally failed to achieve. Repeatedly—but regretfully—I have been forced to conclude that Wilson understands neither the true nature of Lyell's geological beliefs nor the scientific milieu within which he operated. Take, for instance, the origin of river valleys. Wilson praises Lyell for his fluvialistic interpretation of Auvergne in 1828, but he fails to appreciate that Lyell was by no means a convert to the thoroughgoing fluvialism of James Hutton and John Playfair. Although Wilson seems oblivious

to the fact, there were then serious geological objections to the fluvial doctrine, and Lyell, who was only too well aware of the real situation, in 1833 specifically rejected a fluvialistic interpretation of the geomorphology of south-eastern England. Similarly, in complimenting Lyell for his speedy acceptance of the glacial theory in 1840, Wilson forgets that on mature reflection Lyell retreated from his 1840 position and perhaps never wholly returned to its occupation. Time and again Wilson's interpretations are overly simplistic, and his habit on the one hand of lauding those tenets of Lyell's creed that he deems to have been progressive and on the other hand of slurring over those elements that would seem to have been less happy places him in the role of Lyell's eulogist rather than his critical expositor. Most serious of all—and despite the book's subtitle—there is only the most rudimentary attempt to explain the nature of the revolution effected by Lyell's *Principles*. Wilson is content to accept the book's seminal character, and he seems to imagine that his task is satisfactorily executed by tracing the history of the work's authorship and publication and by presenting a conspectus of its contents.

In short, as a picture of the everyday life of a nineteenth-century gentleman of science the book is both interesting and useful; as a study of the intellectual development of the world's most influential geologist it is unsatisfactory and even misleading.

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PHILIP ZIEGLER. *King William IV*. New York: Harper and Row. 1973. Pp. x, 372. \$12.50.

To describe William IV as the most severely limited of all of George III's sons is indisputably a most generous euphemism. And there were serious flaws in the character and behavior of this loquacious boor to justify the constant scolding and urging that formed the bulk of the letters from his parents when he was at sea or on stations in North America and the West Indies. As the third son, he was far enough removed from the throne to justify his father's decision to send him to sea at the age of fourteen and into a milieu woefully inadequate as preparation for the awful responsibility in his

destiny. It turned out to be a bad decision on another count. The young prince could not properly be entrusted with the command of a frigate; his exalted position, on the other hand, made him hanker for the command of the fleet.

Ziegler has written the first biography of William based on the unpublished materials in the Royal Archives and on Aspinall's monumental edition of the papers of George, Prince of Wales. Despite these sources, there is little that is startling or new in the biographical narrative save some random anecdotes confirming the picture that shows itself in other biographies of the king and his contemporaries. The stay in New York, the liaison with Mrs. Jordan, the display of geniality in public, are retold in a lively style and in a manner that makes the royal *gaffes* amusing and even attractive. In his public appearances the "Coconut Head" of England was remarkably like the "Pear Head" on the other side of the Channel.

The king himself destroyed a good part of his earlier correspondence when it was current, and after his death his secretary, Sir Herbert Taylor, destroyed the bulk of the official correspondence. William IV's private purposes are left to the diarists and letter writers of the period, to Croker, Creevey, and Greville and to the fierce partisanship of the Regency and early reform eras. Ziegler has done a commendable job of separating the wheat from the chaff in the delineation of the king's personality.

In light of William's behavior toward the Grey and Melbourne ministries and his bizarre attempts to maintain Peel's first administration in power, Ziegler's encomium of the king as the "first truly constitutional monarch" of Great Britain seems a bit strong. A good case can be made for pushing the prefiguration of Ziegler's conception of constitutional monarchy back to George III in the nineties. And the Byzantine life style of George IV should not blind us to his constitutional reliance on ministers and to his rejection of the heir-apparent cycle that had spawned his early recalcitrance.

Students of Georgian England will also be puzzled to read in Ziegler's prologue that when George III acceded to the throne in 1760, "he found himself King of a country which economically, socially, politically, had evolved little in the preceding sixty years." It is also as harsh as it is inaccurate to dub Sir Herbert Taylor a

"privileged pen-pusher." Indeed, the first man to hold that most delicate and precarious of all court posts, the private secretaryship, appears to be a candidate for further study and investigation as a result of fresh material supplied us by Aspinall from the Royal Archives at Windsor Castle.

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CECIL WOODHAM-SMITH. *Queen Victoria: From Her Birth to the Death of the Prince Consort*. New York: Alfred A. Knopf, 1972. Pp. xii, 486. \$10.00.

Mrs. Woodham-Smith's biography of Queen Victoria, based chiefly on the queen's journal in the archives at Windsor, is a masterly and detailed survey, abundantly documented and vigorous in style, of the period before 1861. Among the new manuscript sources used are the Palmerston Papers formerly at Broadlands; the Conroy Papers at Balliol College, Oxford; the Gibbs Papers (Frederick W. Gibbs was tutor to the Prince of Wales); and the Leiningen Papers of Victoria's half-brother, Prince Charles.

This portrait of the queen is not notably different from that of Lady Longford's *Victoria R. I.* (1964), a book that was also based mainly on Victoria's journal; nor are the estimates of leading politicians—Melbourne, Peel, Russell, Palmerston—much different from those of Lady Longford.

The new biography provides the fullest account we have of Victoria's childhood and early life. Where Lady Longford allotted to the pre-coronation period less than one-seventh of her narrative up to 1861, Mrs. Woodham-Smith devotes a third of her book to it, with special attention to the "Kensington System," which involved the struggle for the control of the young queen waged by her mother and Conroy against Baroness Lehzen. There are, however, no really significant changes in interpretation.

Mrs. Woodham-Smith emphasizes strongly the political and social background of the reign. She dwells at length on the royal dukes, whom Lady Longford treated cursorily. As in Edith Sitwell's *Victoria of England* (1936), there is a full account of the Tolpudde case, even though

it would appear that the queen may never have heard of the affair. The Spanish marriages, the 1848 revolutions, the Crimean War, and the Indian mutiny are discussed at some length. There is even a brief account of the Australian gold rush of 1851. The results of these digressions is that the queen's own personality does not emerge as sharply as in Lady Longford's portrayal.

Mrs. Woodham-Smith's judgment of Victoria is highly favorable. "Honesty, generosity and loyalty were her good qualities. She never bore malice." These qualities are held to "compensate for her displays of emotionalism," of which her married life furnishes a good many examples. There is scant stress on the obstinacy, peevishness, and self-pity that even at this early stage were present, though their full development occurred in the years of widowhood.

"One of the few valid criticisms of Queen Victoria," writes Mrs. Woodham-Smith, "is her lack of concern for social conditions"—a neglect for which Melbourne is held responsible. Victoria's treatment of her unfortunate lady-in-waiting, Lady Flora Hastings ("that odious Lady Flora . . . such a nasty woman") is termed an exception to "the normally generous impulses of the Queen's nature." (Victoria, in fact, wished to have two of Lady Flora's supporters flogged and the whole Hastings family hanged.)

Although the queen's almost pathological immersion in grief for the death of Prince Albert does not come within the scope of this volume, its morbid intensity is clearly foreshadowed in the extravagance of mourning displayed by Victoria after the death of her mother—whom, living, she had disliked and resented for years. On this, Mrs. Woodham-Smith refrains from comment; nor does she attempt to interpret either Prince Albert's extraordinary lack of a will to live or his extreme revulsion from sex—a revulsion felt so strongly that the news of his eldest son's escapade with Nellie Clifden hastened his death (for which, indeed, Victoria held the Prince of Wales responsible).

One awaits with interest Mrs. Woodham-Smith's interpretation of Victoria's relations with Disraeli and Gladstone in the volume that is to follow.

GIOVANNI COSTIGAN
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JOHN CLIVE. *Macaulay: The Shaping of the Historian*. New York: Alfred A. Knopf. 1973. Pp. xiv, 499, xxxvi. \$15.00.

In recent years there have appeared a remarkable number of biographies, spacious in scope and excellence, of personages in modern British history; Blake's *Disraeli*, Woodham-Smith's *Queen Victoria*, Brooks's *George III*, and Gash's imposing and magistral life of Peel, are notable among them. The burgeoning is a little surprising. Leon Edel has suggested that biography is a most amorphous literary form, one with no established canons in which the writer is supposed to be omniscient and infallible. The problems of a historian writing biography are especially great. He has so much latitude, so many possible approaches, so many materials, so great a need for varied kinds of expertness, that if he aspires to completeness and balance he runs serious risks of formlessness, superficiality, and amateurism. It is scarcely surprising that many historians, in particular those of the more strictly analytical variety in the United States, have sometimes regarded biography as a marginally legitimate kind of historical writing.

It is an important part of Professor Clive's success that he has written with a definite and coherent purpose and so achieves a definite and coherent form. He is setting out to explain why Macaulay became a historian and why he was the kind of historian he was. His book stops at the moment when Macaulay began the *History of England* in 1839. Professor Clive has done all a biographer could hope to do: he has written a political biography, a psychological biography, a life and times, and a life and letters, but these are subordinated to his theme and serve to illustrate it. He has achieved not only an imposing work of historical scholarship but a work of art.

Macaulay's personality and ambition emerge as a blending of his own great brilliance and marvelous humor with the influence of an evangelical prig and tyrant of a father and his adoration of (and by) his two much younger sisters. The psychology is unobtrusive and certainly undogmatic, and it persuasively links individual and cultural forces. The changing, cheapening, rigidifying nature of the great intellectual influences of the day—evangelicalism, Benthamism, and romanticism—are suggested with imagination and originality through their effect on an exceptional young man irrevocably

but not exclusively shaped by them. And the author succeeds in making his subject understandable as a personality; we feel we know what it would be like to sit next to him at dinner.

There is much that is, from the professional's standpoint, original. Professor Clive elucidates for Macaulay's age the importance of what we have learned from Caroline Robbins and others—that Whiggery had two faces, one the tradition of the Commonwealthmen that led to Fox and the American Revolution, the other that produced the Venetian oligarchy. The Whig interpretation is not so simple as it is sometimes made to sound, and there was nothing simple, either, about the indoctrination and mind of the Tory-turned-Radical-turned-Whig who wrote the *History of England*.

It is interesting, too, to find how reasonable and indeed prophetic Macaulay's views on Jews, Indians, blacks, and women, sound in the second half of the twentieth century—and not less interesting to find how narrow was his view of the lower orders and their proper political role and how practical, indeed cold-blooded, his determination to overcome, in a society still potently oligarchic, the liabilities of a lack of pedigree and fortune. Similarly, there is illumination (at least for those of us who know more about the history of the United Kingdom than of India) of the minute on Indian education, so much discussed and reprobated. Clive makes no excuses for what turned out to be the defects of the educational system that Macaulay, a European and Liberal, if a singularly flexible one, supported; but Professor Clive shows that the decision for Westernization was the result of long-developing controversies in India itself and that it was wanted, indeed demanded, by educated Hindus of Calcutta. Most important of all, he has shown us the really remarkable extent to which Macaulay contributed to what we now understand as "history"—the systematic analysis of society as a whole, the belief that men are least as likely to be shaped by their culture as to shape it—a major part, in short, of the intellectual apparatus of subsequent generations.

Only the captious would seek out defects. There is, perhaps, a problem about the audience for which the book is intended. Some of the background is explained in rather elementary terms, as if the book were intended for the

general public, but in fact it is only readers with a fair degree of expertness who could understand most of it or—it must be said—be sufficiently interested to read it at all, despite the author's elegant style and even more elegant wit. There is perhaps, too, a slight tendency to applaud Macaulay for percipience while apologetically explaining the limitations of his understanding when he was, from the somewhat telic point of view of contemporary judgment, wrong. But no biographer would, or could, undertake so large a study of someone he did not admire, and Professor Clive never fails in his critical detachment. He has not merely written a very important biography, but he has contributed to our understanding of what a biography can and should be.

LAURENCE D. LAFORE
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STANLEY PIERSON. *Marxism and the Origins of British Socialism: The Struggle for a New Consciousness*. Ithaca: Cornell University Press. 1973. Pp. xiv, 290. \$10.75.

It is often regarded as a historical paradox that Marxism should have made so little progress in Britain, the first major industrial country, where Marx himself lived for so much of his working and writing life. The story has largely been told in the past in terms of the little socialist organizations that came into existence in late nineteenth-century Britain, but A. M. McBriar has written a careful study of the influence of Fabianism up to 1914. There is a need for an equally careful study of the Marxist minority itself and its influence, to establish not only who belonged to it but also what aspects of Marxism they embraced or indeed had the opportunity of embracing—that is to say, what works were translated into English or at least, in view of the average Englishman's inability to cope with German, into French.

Professor Pierson's new work has evidently been in preparation for a good many years, and it is for the most part a careful and accurate work; but it is disappointing to find that it stops short at 1900 and is so largely devoted to surveying ground already covered, without any fresh sources or new insights. Dr. Pierson's method involves the use of a series of short biographies of individual socialists, and although this results in a certain novelty, to be

sure, it is bound to be somewhat confusing to anyone who uses the book as an introduction to British socialism. On the whole the author seems more interested in the Ethical Socialists and the "Religion of Socialism"—including the Labour churches—than in Marxism; but this is only an inference that the reader may make, as there is no critical bibliography and indeed no criticism of sources at all, in spite of the discrepancies that exist among them. The book is also curiously Anglocentric: the sources at Madison, Wisconsin, for instance, have not been used, and we are not told anything about Edward Bellamy's *Looking Backward*, to which William Morris's *News from Nowhere* was consciously written as a reply. Of course the Labour party, founded in 1900, owed relatively little to socialism and hardly anything to Marxism; and if students are to be encouraged, as the blurb suggests, to use this book to "illuminate . . . the origins of the Labour Party," they should perhaps notice that there is a major slip on page 209, where it is suggested that a trade-union fund came into existence in 1893 to finance parliamentary candidatures.

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VINCENT ALAN MCCLELLAND. *English Roman Catholics and Higher Education*. New York: Oxford University Press. 1973. Pp. x, 453. \$22.50.

By the end of Victoria's reign the number of Roman Catholics in England and Wales was still small, comprising under five per cent of the total population; but the growth was nevertheless impressive, for Catholics had doubled in the course of the nineteenth century. At no point and in no manner was it or had it been a homogeneous population, neither occupationally, nor ethnically, nor from the standpoint of social status or income. Not being homogeneous in these ways, it could hardly be so educationally. The Catholic population was also divided according to connections maintained with prominent Protestants or according to the influence sought and enjoyed in Rome. And of course there were religious and political differences. Into this already highly stratified and divided community came yet another group, small but potent and dogged—

the famous converts from the Oxford Movement, some of them fashionable, some well connected, and all of them active and bursting with ideas for the progress, spiritual and material, of the Roman Catholic community in England. In one way or another most of McClelland's study is concerned with the activities and ideas of John Henry Newman (or the converts) and forms an interesting addition to the vast existing literature on that talented and annoying figure.

The Catholic leadership conceded that the provision for higher education substantially lagged behind that of the wealthy Protestant establishment. Oxbridge had revived in two waves, other universities were being founded, Anglican public schools grew prodigiously, and national schemes of elementary education were being launched, with Protestants fighting among themselves for control. The privileged and wealthy Catholic aristocracy wanted greater participation in the attractive social and political life of the governing elite, and the growing Catholic middle classes were interested in professional careers. If the Catholic community was to take advantage of its enfranchisement in the 1820s, make the most of the re-establishment of the hierarchy in the 1850s, and maintain its coherence in the face of the overwhelming influences of industrialism, liberalism, and free-thinking, then traditional insularity and educational backwardness had to be overcome. There were new challenges, opportunities, and dangers.

It is one thing to perceive a need for a better educated population (or desire it), yet another to decide which form of education fits the need best. This is where most of the fascinating educational controversies in higher education took place in the nineteenth century, and so it was with the Catholics. The nature of the education provided depended upon the recipients. As it was the flower of the Catholic territorial aristocracy that was mainly talked about, some form of liberal education was the answer. All Catholics agreed that the education provided should be dogmatic enough to combat religious skepticism. There was also the problem of the charm, confidence, beauty, and cultivation of the finest Anglican institutions. The emigrés from Oxford could never forget the dreaming spires. Consequently it had to be decided whether Catholic youth should be in-

sulated from Oxford's pernicious influences by being educated in purely Catholic foundations or whether they could be trusted to attend Protestant institutions with control exerted through separate communal residences. London University, nonresidential and converted into an examining institution, posed no particular threats.

By the opening of the twentieth century the educational situation had changed remarkably, for England had changed. The Catholic community was no longer dominated by the fashionable old families. Newman was gone. The hierarchy was better known and not so severely distrusted, at least at the upper levels of English society. Economic and numerical expansion had produced a more significant Catholic middle class. English Protestantism no longer seemed quite so formidable, as modern scholarship, science, and the attack on privilege and established institutions reduced many of the dangers of "mixed" education. History, however, continued to defeat the converts. Newman's imperial university had come to nought. And even when the ban on Oxbridge was lifted by the hierarchy the dons rose in loyalty to the memory of Elizabeth and defeated a proposal for a Catholic college in Cambridge.

McClelland's clear and highly useful narrative is a story told from on high, being an account of bishops, thinkers, organizers, their letters to one another, their essays, and their communications to Rome. There is fine material here. But a conventional criticism should be made. The problem as seen from the other end is largely unstated. There are relatively few references to the changing character of the Catholic community in general, the changing occupational and geographical distribution of Catholics, and the pressures exerted directly or indirectly on the hierarchy. Nor are the Irish in England visible. The state and nature of religious belief within the Catholic community at large is passed over. The reader has only a slight idea of what is happening below, and therefore the historical relationship between education, social change, and religious leadership within the Roman Catholic community in England cannot be fully grasped.

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GARETH STEDMAN JONES. *Outcast London: A Study in the Relationship between Classes in Victorian Society*. New York: Oxford University Press. 1971. Pp. xiv, 424. \$15.25.

"Outcasts," "aliens," the "residuum," the "dangerous class," the "undeserving" or "unrespectable" poor—under one name or another, this group was a persistent feature of nineteenth-century England. It troubled contemporaries and it continues to defy historians. We still have not determined the size and boundaries of the group, the relations of subgroups within it, or their relations to the rest of the population. How much mobility was there within this "class" or between it and the "working class" or "respectable poor" outside? In what grey areas of legality or illegality did the "street-folk," as Henry Mayhew called them, operate? We know of seamstresses falling into prostitution, but how irrevocable was that fall, how fateful in their own perceptions of themselves and in the judgments of others? To what extent did this world perpetuate itself from one generation to another?

The questions multiply, and this book is a serious attempt to come to an understanding of some of them. But only some of them; some are precluded from being asked by the framework of analysis adopted here. For the "Outcast London" of the title, which inspires these and similar questions, is used by the author ironically, to express what he takes to be the "mythology" of Victorian England, a mythology designed to conceal the true social "reality." That reality, as he sees it, was nothing more or less than unemployment: structural, cyclical, and seasonal unemployment that gave rise to "endemic forms of poverty" for a considerable class of "casual labor." In addressing the problem of casual labor in these terms, the author has amassed a considerable and valuable quantity of information about the nature and extent of unemployment, the living conditions (especially housing) of those subject to one or another variety of unemployment or underemployment, and the effects upon them of various attempts, private and public, to cope with and alleviate those conditions.

One concludes this book grateful to Mr. Jones for the lively intelligence and the commendable industry he has brought to his subject. But one may also have some reservations about a conception of social reality that sees the

problem entirely in terms of casual labor and unemployment. This is not the familiar complaint of the reviewer who wishes the author had written a different book. It is an objection that goes to the heart of the book, and perhaps also too much recent writing of history. For the author's presumption, like that of many historians, is that he can see the problem more clearly and objectively than contemporaries could. They were caught up in the "mythology" of moral judgments; he can expose the "objective" economic reality. They saw things through the "distorting lens of middle-class aspirations to gentility," through the "deformations" of the liberal "ideology"; he is free of all such distortions and deformations.

The difficulty, of course, is that Jones is dependent upon those "mythologizing" contemporaries for much of his evidence. He must, therefore, use that evidence selectively, picking and choosing those bits that support his thesis. In the present case, this means praising Mayhew, for example, for seeing "clearly" what others presumably failed to see: the importance of seasonality in the London economy, the connection between seasonal labor and low wages. But if Mayhew saw so clearly on these subjects as to warrant extensive quotation, why does Jones never quote him on other subjects that preoccupied Mayhew far more: the peculiar physique and mentality of the "nomadic race" of street-folk, a race that had a positive "repugnance to regular and continuous labor," an "inability to perceive consequences," a "passion for stupefying herbs and roots," an "immoderate love of gaming," an "absence of chastity," and more, very much more, in the same vein, all illustrated and documented at great length in the four volumes of *London Labour and the London Poor*.

Another thesis of this book is that the fears about the "outcast" class grew as the century progressed, reaching their height in the 1880s. Here one is caught between admiration for the author's subtlety in interpreting the evidence and wariness of that subtlety. The dock strike of 1889, for example, might have been expected to bring those fears to a climax. But since it was perfectly evident that contemporaries did not, in fact, panic in the face of that strike—indeed, that much of "middle-class London" was enthusiastically in favor of the strikers—

Jones ingeniously concludes that the strike came as a "cathartic release from the social tension of the mid-1880s." Perhaps so. Or perhaps not. Perhaps the tension was never as severe as Jones made it out to be. Perhaps we should take more seriously, even respectfully, the work of philanthropists like Octavia Hill and the Charity Organization Society, or the legislation of the seventies and eighties, in alleviating that tension. Moreover, in a work where chronology is so central to the thesis, one is made uneasy by the casualness with which Jones lumps together or quotes indiscriminately from Mayhew (most of whose work dated from 1849–52) and from Charles Booth (whose data was collected almost forty years later).

These demurrals testify to the provocativeness and importance of *Outcast London*. It is a book all social historians will use, and some will quarrel with, for a long time.

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BRIAN BOND. *The Victorian Army and the Staff College, 1854–1914*. London: Eyre Methuen. 1972. Pp. xv, 350. £5.50.

Generals represent the elite of the military profession. How Britain selected this elite from the officers who attended the Staff College at Camberley in the late Victorian era and how these professionals became the commanders of the army by World War I are the subjects of this thoughtful work.

A new sense of professionalism pervaded the military world by the 1850s, due to changes in warfare during the French Revolution, the elevation of standards for prestigious civilian occupations, and the screening of candidates for commission. Warfare was now increasingly scientific. Technological developments effecting the movement of troops made it apparent to reforming war ministers that the Wellingtonian army had to be modernized. The day of the blundering amateur-at-war was over at Balaclava.

After the Crimean War, the curriculum at Camberley was improved to make the courses in tactics, logistics, and strategy more realistic. Yet graduates of the college lacked prestige, and the commander-in-chief, the duke of Cambridge, was critical of educational innovations. After

the Boer War, however, the army's new breed of intellectuals won some victories. The reactionary Cambridge had retired in 1894; the master of the small colonial war, General Sir Garnet Wolseley, had demonstrated the value of staff specialists; and competition for appointments to the college became keener. Furthermore, the South African war revealed serious defects in the organization of the army, deficiencies that might have been corrected by a general staff, which was not established until 1906. Due to the entente with France and the growing animosity of Germany, the college, now the nursery for the general staff, had a definite purpose as its students pondered the possible deployment of the British Expeditionary Force to the Continent. Staff College training, in fact, became virtually obligatory for coveted appointments on the eve of World War I.

The test of these experts was on the French and Belgian battlefields in 1914. Although Bond notes the difficulty of assessing the college's influence during the early campaigns on the western front, he admits that the disastrous consequences of ill-coordinated offensives in 1915 demonstrated the inability of British generals to adapt their tactical doctrine to static trench warfare.

This is a useful work. Yet the excessive use of ponderous quotations occasionally mars the narrative; the possible influence of other national staff colleges is slighted; and the British army is described as seemingly unaffected by the gradual transformation of the contemporary social structure. And surely the inclusion of a collective biography is necessary to categorize this elite. However, the author has presented his thesis in an illuminating manner, and he has made a definitive contribution in clarifying the influence of the Staff College on military thought.

RICHARD L. BLANCO
State University of New York,
College at Brockport

JOSÉ HARRIS. *Unemployment and Politics: A Study in English Social Policy, 1886-1914*. New York: Oxford University Press. 1972. Pp. x, 411. \$24.00.

This book bears all the marks of a revised doctoral dissertation. It is earnest, contentious,

heavily footnoted, and slightly tedious. But nevertheless it also tells a great deal about British efforts to grapple with the problem of industrial unemployment in the three decades before the First World War. Perhaps the chief fault is not the narrowness of the treatment, which is after all characteristic of works of this kind, but that even within the close limits which the author set for herself, she attempts to do too much. There are endless extracts from easily available documents. The work of the Webbs on the Minority Report of the Royal Commission on the Poor Law is assessed yet another time. The varied failures and successes of the Unemployed Workmen Act of 1905 are detailed and cataloged. In all of this the spectacular political daring that was the experiment with unemployment insurance is almost lost. No one ever says that Churchill and Llewellyn Smith were in fact taking a very large political chance in compulsory, treasury-supported—for that in fact is what it was—out-of-work compensation paid as of right. It was not an idea plucked from the flood of writing about what to do with the destitute poor left over from the Poor Law, nor was it a response to the Labour party's perennial Right-to-Work Bill. It came, as does most important legislation, from hard politics and personal ambition. It bore about the same relation to the generation of ideology that had preceded it as does NASA to good science fiction writing. Mrs. Harris knows this and says so on page 362, at the end of her summary, by pointing out that there was "little conscious reference to theory in Liberal reforms." In doing so she makes the first half of the book, which is about theory, irrelevant.

This is still a book that was worth writing. One could wish that the author, hard-working and serious, would remember that the New Liberalism has been worked and reworked in the last few years and that a new footnote does not necessarily change an old fact. Everyone agrees that Churchill and Lloyd George were not supermen, that much that they accomplished had been proposed by others, and that the welfare state did not come into being with the National Insurance Act of 1911. But the work of these men did lay the foundations for the present institutions of British social security, and if historians will keep their sense of detachment, perspective, and above all their sense

of humor, they will find in the New Liberalism a political philosophy that is at once coherent and very English.

BENTLEY B. GILBERT
University of Illinois,
Chicago Circle

RICHARD PRICE. *An Imperial War and the British Working Class: Working-Class Attitudes and Reactions to the Boer War, 1899-1902*. (Studies in Social History.) Toronto: University of Toronto Press. 1972. Pp. xiii, 279. \$11.50.

Here is an attempt to show that the idea of working-class enthusiasm for the Boer War of 1899-1902 was no more than a myth. Myth it might have been, but that is not really proved by the evidence produced by Dr. Price, who has taken as his principal samples the membership of workingmen's clubs with a strong background of radicalism. Of course, it is extremely useful to have the middle-class leadership of mob jingoism amply demonstrated and to be steered firmly away from the realms of untruth. It is, nevertheless, only a partial service to take away "2x" of falsehood with one hand, only to replace "x" of it thereafter. Some emphasis on the metropolis was certainly justified. London was the scene of the most jingoistic outbursts. Even so, the provinces should have been taken much more into account.

By the end of the nineteenth century the Liberals were supported by the majority of British workingmen, but the Conservative minority remained substantial. That minority would certainly have tended toward jingoism, albeit mainly led by its social superiors. Then, too, there certainly were large pockets of Liberal Imperialists dotted all over working-class areas. These, too, would be for the Boer War. Dr. Price's initial analysis was faulty, if in fact he ever made one. One suspects he did not, because the book is essentially episodic. Nothing in it is uninteresting or badly written. On the contrary, much of it is actually fascinating. At the same time it is often irrelevant, assuming the theme to be the one set out about working-class attitudes. The tables presented for comparison are not always suitably chosen.

The actual course of the failure of radicalism to combat the war policy and its propaganda, the accounts of the origins of the working-class

clubs, and the course of the 1900 general election and the makeup of the jingo crowd, not to mention the details as to recruitment, all make worthwhile reading. While the theme of the book is unsustained, its details frequently illuminate. While there are some mistakes in details, the general picture is one of commendable accuracy.

MICHAEL HURST
St. John's College,
Oxford

BERNARD GAINER. *The Alien Invasion: The Origins of the Aliens Act of 1905*. New York: Crane, Russak and Company. 1972. Pp. 305. \$12.75.

The recent furor over legislation to regulate Commonwealth immigration to England has generated two studies of the British response to Russian Jewish immigration at the end of the nineteenth century. John A. Garrard's *The English and Immigration 1880-1910* (1971) contends that Liberals and Socialists adhered to traditional ideals and opposed restriction, despite the vocal opposition of a portion of their supporters, with greater consistency than the Labour party did in the 1960s. Covering much the same ground, Gainer, in a more incisive but less satisfactory work, focuses on the agitation that produced the Aliens Act of 1905, a measure that sought to restrict entry into the United Kingdom of those deemed socially undesirable.

Antialien sentiment derived from a misconception, fostered by inaccurate and occasionally distorted statistics, that England was being subjected to an invasion by a horde of unskilled social misfits who displaced English workers and congregated in overcrowded East End London slums. In fact, as *The Alien Invasion* reveals, the number of immigrants was relatively small—roughly 80,000 from Eastern Europe by 1901. While it is true that the Jewish refugees gravitated toward the sweated workshop trades, they aggravated an already existing problem, identified decades earlier by Mayhew and Kingsley, and did not create a new one. It was increasing provincial and foreign competition, as well as mechanization, that led to unemployment in cheap clothing manufacture and the lack of state supervision that permitted the sweating system to flourish.

Gainer explores the varied sources of anti-alienism—working class insecurity, latent anti-Semitism, protectionism, and anxiety about ebbing national efficiency—that could be tapped by proponents of restriction, like Arnold White and Major William Evans-Gordon, but he fails to explain adequately its translation into political action. By his own estimation the antialien phenomenon remained geographically limited to London, where organizations like the British Brothers League, which advocated regulation as a panacea for economic ills, never mobilized the dedicated following typical of Victorian pressure groups. He suggests that the Conservative party endorsed restriction as an expedient palliative for working-class grievances. It was able to court working-class voters by prescribing a remedy for unemployment that would not impinge upon the economic interests of its wealthier supporters. But there is little evidence that the alien issue was electorally significant even in London. Tory strength in working-class constituencies was greatest in 1895, when party leaders were indifferent to the immigration question, whereas six East London seats were lost in 1905 immediately after the passage of the Aliens Act. Restriction gained the party little support in the provinces, and the coupling of antialienism with protection probably damaged its electoral prospects in London.

Although carefully researched and abundantly documented, Gainer's work suffers from an excessively narrow focus. The author says little about the response of the Jewish community to the new arrivals and tends to underestimate the British commitment to the right of asylum and to unrestricted entry. Too much of the book is devoted to a recapitulation of parliamentary debates and Royal Commission testimony, and its topical organization lends itself to a good deal of repetition.

F. M. LEVENTHAL
Boston University

HAROLD MACMILLAN. *Pointing the Way, 1959-1961*. New York: Harper and Row. 1972. Pp. viii, 504. \$15.00.

This is the fifth volume of Harold Macmillan's memoirs. Inevitably, comparisons suggest themselves with what Winston Churchill wrote about his own stewardship of Britain's affairs. There

are some obvious similarities between what Macmillan writes and what his more illustrious predecessor chose to say. Each is preoccupied with the "special relationship" with the United States; both continue to concern themselves with what was once called the Empire and had become the Commonwealth; the state of the alliance is very important to them. Such similarities, however, are superficial when compared with the differences between the two memoirs. Churchill lived in heroic and dangerous times; he wrote of great events. The events Macmillan describes are great only in his own memory. Much less depended on what he did (or said) than he realized. The events of 1959-61 seem curiously more remote to the world of 1973 than do those of the years 1939-45. Macmillan constantly worried and fretted about the "crises" of his time; he alludes to his anxiety much more frequently than Churchill ever did. While it would be ungenerous to say, with the advantage of historical hindsight, that he ought not to have worried so much, there is a lack of self-awareness here that is genuinely disconcerting. The ball was not as frequently in Macmillan's court as he imagined it to be. In 1960 Macmillan produced a document that he half-jokingly referred to as "The Grand Design." That document seems more positively old-fashioned, parochial, and out of date than almost anything that Roosevelt and Churchill ever fashioned.

These statements should not be taken to imply any major dissatisfaction with the book. They simply emphasize how quickly the events of the late Eisenhower and early Kennedy years have receded into history. Macmillan, in this volume, as in the others he has written, shows himself a master of English prose and a deft portrayer of political personalities. Despite his genuine liking for Eisenhower, he cannot conceal the dismay he felt at the ineptness shown by the president in his handling of the U-2 episode, which contributed to the failure of the Paris summit meeting of 1960. Eisenhower, in Macmillan's eyes, had sterling qualities as a man; this did not make him a successful president, particularly in his ill-starred second term. For de Gaulle there is grudging admiration and a great desire to emphasize the excellent rapport that developed between the two. In many of de Gaulle's remarks, reproduced by Macmillan,

there is an obvious tribute intended—both to de Gaulle's insight and to his intelligence; the decisiveness of the man clearly appealed to Macmillan. On Adenauer he is much more circumspect; the fundamental distance between the two is never concealed or denied. As for Khrushchev, Macmillan represents him as a ham actor; truculent, boastful, vain, and not always very consistent. Some of his most interesting remarks he reserves for John Kennedy. Having not known him before he entered the White House, he emphasizes the rapidity with which he established a close and confidential relationship with him. He is pleased that Kennedy could be an "intellectual" as well as a political companion. Kennedy is generally portrayed in two poses: the inveterate questioner and the callow youth soliciting advice from the man of experience. Kennedy, overwhelmed and shocked by his meeting with Khrushchev in Vienna, is represented as wanting to meet with Macmillan alone so that he can learn from him. Many questions suggest themselves. Would Macmillan have written in this way if Kennedy had lived? Was the relationship quite as one-sided as he makes it appear? Was there no guile in Kennedy? Finally, why is it easier for Macmillan to find chinks in Kennedy's armor than in that of any of his British colleagues? His comments on his own associates make them all appear to be wise, generous, and farseeing.

It is curious that one's interest in the book is almost wholly with Macmillan's depiction of individuals. The political issues appear much less compelling. Rarely does Macmillan contribute very much to our knowledge about individual events. There is relatively little analysis of the nature of the rifts that existed within the alliance, and the evidence on Allied relations with the Communist world is superficial. The book is entertaining; in places it is candid; it is the work of a civilized man. But it all seems to have happened so many, many eons ago.

STEPHEN R. GRAUBARD
Brown University

DAVID MCKIE and CHRIS COOK, editors. *The Decade of Disillusion: British Politics in the Sixties*. [New York:] St. Martin's Press. 1972. Pp. 250. \$9.95.

The sixties are still much too close to us to handle without some unifying and simplifying idea, and perhaps for Britain "disillusion" will

do as well as any other. However, the authors of these essays have not worked out what they mean by the word. Contrast Leslie Stone's neat and competent account of the British withdrawal from the illusions of empire and world power, which still flourished in the late fifties, with Peter Sinclair's gloomy demonstration that each of the economic policies tried in the sixties led to failure, and it emerges that these two creditable essays differ on whether the British had successfully got rid of their illusions or had an inexhaustible supply still to come. The retreat from empire and world power is, of course, too much for a fifteen-page essay, and Stone's handling of the topic makes one hope for a book. Almost the only other person with the elements of a book in his essay is David McKie, who writes about "The Quality of Life" in a way that shows he cares about both technology and the environment (although *The Limits of Growth* came a little later, he could probably write about it sensibly), and he also concisely fits in the relaxation of so many over-moralistic laws under Labour.

There is an informative essay—perhaps too solid to wish it longer—about immigration by Roy Hattersley. A curious essay on "Social Welfare and Housing" certainly makes one wish for more about housing, because it gives only two pages to a subject of more or less universal interest, but Anne Lapping rather clearly feels that the social services are a fragile plant that would suffer if discussed in terms of disillusion. Education might have seemed quite a good subject for disillusion, but Brian MacArthur has the same sort of feeling that education ought to be defended. I hope his dreams come true, but he is not writing within the framework of the title.

Victor Keegan's study of industry and technology has enough disillusion for anyone; nihilistic pessimism is perhaps the word for Harold Jackson's suggestion that what the British really want to do about Ulster is to leave and let them fight like the Kilkenny cats. Hugo Young has written an essay about the various pressure groups for good causes that flourished in the sixties, but it is confused by his conviction that nobody less respectable than the Confederation of British Industry ought to put pressure on the government. As a result he writes about organizations like the Campaign for Nuclear Disarmament in terms so

apocalyptic that they would make sense only if CND had wanted to become the government. Chris Cook is much more reasonable about minor parties, though it seems rash of him to write of Liberal by-election successes as the result of disillusion and then discuss their 1970 general election losses as though the British would never again be disillusioned with their politicians.

This may all seem rather a muddle, but it might be cured by a strong central essay. Unfortunately the main essay (John Barnes on "The Record") is also the worst. Apart from the bad grammar and the shaky economics, it is written like a newspaper story. Faced with a deadline, obliged to write without knowing how the story ends, a newspaperman cannot reflect or analyze much—if only because next morning may show he was wrong. With less cause, Barnes writes from one headline to the next, and instead of analyzing, he prints the figures of the opinion polls. The events of the sixties are mentioned, but they are submerged by the frenetically allusive style, so that only someone who knows what happened in Britain in the sixties will be able to follow the essay. And someone who knows will not need to read Barnes.

TREVOR LLOYD
University of Toronto

W. A. MAGUIRE. *The Downshire Estates in Ireland, 1801-1845: The Management of Irish Landed Estates in the Early Nineteenth Century*. New York: Oxford University Press. 1972. Pp. vi, 284. \$19.25.

With the appearance of Dr. Maguire's excellent book, historians need no longer lament the absence of useful works on Irish agricultural history. He has thrown much light on this hitherto obscure subject by a study mainly confined to the resources and management of the vast Irish property (some sixty thousand acres) of the third marquis of Downshire. To limit oneself to a single estate may prompt critical murmurings in sophisticated circles about statistical significance. But Dr. Maguire has taken care to examine an estate that not only was very large but also comprised several properties both in the north and south of Ireland, thus encompassing important regional differences, and, what is more, one that has left an extraordinarily rich archive of estate papers. He has,

moreover, put these manuscripts to very good use. He has read them in the light of what he has learned from less formidable manuscript collections of eight or nine other estates, from a large contemporary literature, notably the Devon Commission Report, and from a careful study of the work of English estate historians (none of whom, curiously enough, has ever written a large-scale study of a single estate). The happy results are that he settles down on the right topics, asks the right questions, and thereby opens up enough fascinating lines of thought to inspire doctoral candidates for a long time.

Perhaps his most startling conclusion is the following: "The striking fact that emerges from a study of estate records is the importance of the agricultural crisis at the end of the war with France. It is of course true that the Famine caused, or at least brought to a head, a financial crisis for many landlords, but the years 1815-20 rather than 1845-50 mark the real watershed between the land situation as it was in the eighteenth century and the crisis of the late nineteenth century" (p. 250). The eighteenth century had been marked by the prevalence of the long lease that in Ireland, as Dr. Maguire contends, helped to encourage the subdivision of tenant holdings and the spread of retrograde farming practices. The post-1815 fall in agricultural prices nurtured the beginning of landowners' resistance to these trends, in which the marquis of Downshire took a leading part. Impelled largely, it would seem, by an un-Keynesian abhorrence of debt, this otherwise unremarkable nobleman built a system of estate management quite as businesslike as the most businesslike of great English estates. In analyzing the marquis's achievement, Dr. Maguire has provided a model of what needs to be done not only for Irish but for English estates as well, wherever the documentation is so abundantly available.

DAVID SPRING
Johns Hopkins University

W. B. STANFORD and R. B. MCDOWELL. *Mahaffy: A Biography of an Anglo-Irishman*. Boston: Routledge and Kegan Paul. 1971. Pp. xiii, 281. \$10.00.

John Pentland Mahaffy was one of the more interesting Irishmen of the past century. His

biographers describe him—without exaggeration—as “historian and philosopher, man of letters and musician, conversationalist and controversialist, sportsman, publicist, diner-out and don.” Born in 1839 and an authority on Hellenistic Greece, he was a scholar of Trinity College, Dublin, and its professor of ancient history, and he ended his life in 1919 as its provost.

This biography deals expertly with his life as a scholar, describing his love of Greece; his published works, which did much to perpetuate public interest in the classical world; and his controversies with other scholars in Trinity and in Oxford and Cambridge. He was arrogant and inaccurate as a scholar and was never accepted as a great authority; he was a snob in social life as well as in academia, but his honesty and courage atoned for much.

This book is very well written and is always entertaining. It seems much more the work of Stanford—a professor of Greek—than of McDowell, a historian. The historical background is scarcely dealt with. Mahaffy might as well have been a *don* in Oxbridge or Edinburgh for all that is told of his relations with the Irish population. His attacks on the Gaelic Revival and Home Rule are only lightly etched in. He did much harm to his college by his arrogance in this context. The Catholic population was only emerging from second-class citizenship in the nineteenth century and suffered from an inferiority complex in regard to the Protestant Ascendancy. It felt Mahaffy's disdain keenly and, naturally enough, blamed it on his college. In 1921 this new *de facto* Catholic democracy took over. Thanks largely to Mahaffy, Trinity found itself isolated and only gradually found its feet in the new Ireland. Its survival was due as much to the religious tolerance of the new rulers as to its own efforts. Even though he did his college harm, Mahaffy must be acknowledged as a distinguished scholar and Irishman.

MAURICE R. O'CONNELL
Fordham University

ROBERT M. ISHERWOOD. *Music in the Service of the King: France in the Seventeenth Century*. Ithaca: Cornell University Press. 1973. Pp. xiv, 422. \$17.50.

Musical topics are dealt with all too rarely by real historians. Musicologists often lack the

kind of professional training in historical method so essential for perceptive research in matters political, sociological, and esthetic, with the result that much writing about music restricts itself to the purely analytic. On the other hand, a historian with a background in practical music can offer an abundance of information seen from a new angle, as Woodfill did in his admirable study of musicians in the time of the Tudors and Stuarts. Professor Isherwood, also a practical musician, provides a comparable service for those fascinated by the extraordinary state of musical affairs in seventeenth-century France.

Although he begins with a useful but necessarily brief account of musical philosophy and continues with a lively summary of court music and masquerade in the Renaissance, the author devotes by far the greater part of his book to the operas, ballets, and *divertissements* written by Lully for the amusement of Louis XIV and his scintillating entourage. The relationship between a musician of humble origin and a king who shone as the sun in the heavens was a strange one, and it has often been discussed in works that vary from the scholarly to the scandalous. Here we see things in perspective. Lully is portrayed as a man anxious for personal advancement, which he obtained successfully by doing precisely what was expected of him. Artistically involved in serving an absolutist monarch, Lully was absolutely ruthless in all his dealings. His creations, in which he took an active part as composer, dancer, and director, were in some ways similar to present-day musicals, for the accent was on spectacle, costume, choreography, and melody. And just as the modern composer of musicals frequently works as the chief of a team, so Lully sketched his scores and then handed them over to secretaries, who filled in harmonies and added details in orchestration.

Lully may have been the dominant musical figure of his day and age, but he was far from being the finest musician. Charpentier's effortless superiority as a craftsman and as a contrapuntist drew from the king a veritable paean of praise when the opera *Médée* was first performed. Its prologue, of course, contained the accustomed element of flattery, but Charpentier could claim that he was serving the monarch in the expected manner, just as he wrote special

music for the dauphin and for other eminent members of the court. Although Professor Isherwood's flowing style and solid grasp of sources command our attention, many readers would gladly exchange some of the complicated opera plots for a closer look at other figures such as Charpentier, Dumont, and Robert. The Abbé Robert, for example, may be almost a biographical cipher, but his published volumes of motets show him to be a French Gabrieli in his skillful manipulation of orchestra, soloists, and choruses. In many ways the music of the royal chapel also deserves our respect and study, and the account of it in the chapter on ceremony and celebration could go deeper. Perhaps the author will follow up this excellent foundation study with others dealing with Lully's contemporaries.

DENIS STEVENS
Columbia University

LIONEL GOSSMAN. *French Society and Culture: Background for 18th Century Literature*. Englewood Cliffs, N.J.: Prentice-Hall, 1972. Pp. viii, 149. \$6.95.

This book is briefer and more condensed than John Lough's *An Introduction to Eighteenth-Century France* (1960)—probably the standard introduction to eighteenth-century French society for English-speaking students of its literature—but also more sophisticated in its conception and ambitious in its scope. Lough aimed simply to provide students of literature with a background knowledge of society, and he did this very well. Gossman invites them (albeit very tentatively) “to relate the cultural forms by which a society seeks to attain consciousness of itself and of the world to the organization by which it secures subsistence and continued existence” (p. vii). The result is a challenging piece of work, introducing students to intellectual approaches and concerns that have come to the fore in the past decade.

Professor Gossman begins with an excellent brief sketch of social classes and conditions which serves to introduce the thesis that “the Enlightenment presented itself in universal terms, but the concrete reality in which it was embedded was the struggle of one minority social group to wrest power from another minority social group” (p. 38). The exact nature

of this struggle between the bourgeoisie and the privileged orders is not really elaborated either in this first chapter or in the rather scanty second chapter on social change and attempts at reform. Instead, Gossman goes on to summarize Labrousse's thesis concerning the crisis of the French economy in the 1770s. This summary suggests that social tensions may have increased in the last decades before the Revolution, but it says little about the nature of the struggle that Gossman postulates as the concrete background to the emergence of enlightened ideas (that is, before 1770). Furthermore, while the account of attempts at fiscal reform is brief but informative, the discussion of economic reforms is too rudimentary to sustain the argument that the economic prosperity of the mid-century led to “the progressive withdrawal of the state from the economic sphere and the collapse or at least the weakening of established political and economic institutions and values” (p. 57). This withdrawal (sporadic, perhaps, rather than progressive) probably had as much to do with relatively long-term developments in the political institutions and values of the monarchical state as it did with immediate economic conditions. The omission of any discussion of such political developments weakens this section of the book.

In the remaining two chapters, however, Professor Gossman comes into his own. The third chapter—complicated, uneven, far too compressed to be readily grasped in this form—is, in essence, a sketch for a brilliantly rich book he must surely go on to write, analyzing the strains and tensions of the French Enlightenment as a bourgeois world view, in categories suggested by Franz Borkenau's *Der Übergang vom feudalen zum bürgerlichen Weltbild* (1934). Many characterizations of the Enlightenment as a “bourgeois” phenomenon seem to remain rather sterile: they label the phenomenon in a way that tells us relatively little about it. Gossman's analysis, on the contrary, suggests some real insights into the ideas of the philosophes. His general characterization can and will be debated. But his discussion—involving such themes as the reception of Lockean ideas in France, the strains in materialism, and the “pessimistic” sources of the radical critique of society that threatened enlightened views—remains very suggestive.

Finally, Gossman concludes with an analysis of some recent approaches to the sociology of literature and a tentative exploration of the sociology of the literary form in eighteenth-century France. For historians who probably know relatively little of the work of literary scholars in this field, this will be one of the most interesting chapters of a stimulating little book.

KEITH M. BAKER
University of Chicago

V. G. REVUNENKOV. *Parishskie sankiuloty epokhi velikoi frantsuzskoi revoliutsii* [Parisian Sans-Culottes at the Time of the Great French Revolution]. (Leningradskii Ordena Lenina i Ordena Trudovogo Krasnogo Znameni Gosudarstvennyi Universitet imeni A. A. Zhdanova.) [Leningrad:] Izdatel'stvo Leningradskogo Universiteta. 1971. Pp. 174.

This small volume by Professor Revunenko of Leningrad State University, a senior Soviet specialist on modern French history and on the history of revolutions in modern times, was designed for students, graduate students, and professors at all levels of Soviet higher education. It relies heavily for its factual basis on the works of Saboul and Rudé and on Soviet editions in Russian of the collected works of Marat, Danton, Robespierre, and of translations of the works of Lefebvre, Mathiez, and Thorez. So far as one can tell, Professor Revunenko has not conducted research in France or visited that country.

Parishskie sankiuloty reflects the growing Soviet interest in French history, which began to burgeon about fifteen years ago, a decade or so before a similar interest in American history appeared. This book illustrates the application of pious Marxist-Leninist formulas to historical developments. Revunenko argues that the revolutionary bourgeoisie and the national masses formed a block, but that the Jacobin bourgeois dictatorship then destroyed the power of the sans-culottes, liquidated the national elements that had led the August 10, 1792, uprising, and, as an instrument of the bourgeoisie, defended that class from the popular masses.

Revunenko cites Marx and Engels to support his view that 1793 represented the dictatorship of Paris and the bourgeoisie over France. He notes that Lenin identified the death of the

"first revolution" with the Jacobin dictatorship and that this was natural and indeed inevitable, since the Jacobins established a dictatorship of the bourgeoisie in the conditions of a bourgeois revolution. On the other hand, the Bolsheviks, according to Lenin and Professor Revunenko, united dictatorship with democracy and waged a civil war while providing for wide participation of the masses in state affairs. They provided Russia with its first Soviet constitution, which strengthened the dictatorship of the proletariat, denied the "exploiters" the right to elect or be elected, and simultaneously established a "democracy." In short, the enemies of socialism were not allowed to use "Soviet democracy" for their interests, while the rights of all, or at least of the workers, were protected and even expanded. The Russian Revolution was therefore more democratic than the French Revolution.

An American finds it difficult to distinguish between these dictatorships. Revunenko is able to condemn the Jacobins and praise the Bolsheviks because to him the goals of the two dictatorships are so different, the ends justify the means, and historians serve political causes.

ROBERT F. BYRNES
Indiana University,
Bloomington

DAVID H. PINKNEY. *The French Revolution of 1830*. [Princeton:] Princeton University Press. 1972. Pp. ix, 397. Cloth \$16.00, paper \$8.50.

Fifteen years ago Professor Pinkney formulated what deserves to be known as Pinkney's Law: the more specialized their research on France the less American historians contribute. Instead of competing on unequal terms with their French colleagues, American scholars, Pinkney maintained, should offer broad interpretations of the French past to American readers. Pinkney's Law, greeted with approving nods, did not deter the noddors (myself included) or their graduate students from continuing to grind out monographic studies. Rational assent was neutralized by the deeply felt conviction that the law applied to the other fellow only. As to budding young scholars, they remained governed by the iron law of dissertation topics. Yet I am convinced that Professor Pinkney was right, and with the courage of his convictions he has now published

this book as a sort of exemplar. Consequently it deserves to be considered on its own merits and as a model to emulate or shun.

The July revolution has been relatively neglected by historians not only in the United States but in France as well. Professor Pinkney, drawing on a considerable memoir and monographic literature—as well as on his own substantial archival research—has produced the only up-to-date scholarly book on the political crisis that toppled the last Bourbon king in France. It is thorough, interestingly written, and well organized—first-rate in every way. Though a narrative of political events from 1829 through 1830 is central, at least three chapters, “Sources of Opposition,” “The Crowd in the Revolution,” and “Purge and Replacement,” stress analysis.

Aside from its greater depth and complexity Pinkney's synthesis does not greatly alter the conventionally accepted picture. When it comes to Charles X and his ministers their bumbling incompetence appears to defy any revisionism. The liberal opposition, never militant in traditional accounts, seems even more timid and passive. Indeed, Pinkney is most convincing in depicting the revolutionaries' artisan background and their autonomy from the parliamentary liberals. He makes short work of the myth—well entrenched by 1848—of a republican revolution confiscated by monarchists. He presents evidence for a strong undercurrent of popular Bonapartism, a much weaker republicanism, and no indication that either faction was seriously organized. The Orleanists won out because of able leadership, skillful propaganda, and Louis Philippe's availability.

Among the more original aspects of the book is the author's survey of the provinces and his assessment of the class character of the revolution of 1830. The provinces, it seems, were restive enough to welcome the overthrow of the Bourbons. As to the problem of the “bourgeois revolution,” the vast administrative purge in the wake of Charles X's fall brought government jobs to thousands (most of them former Napoleonic officeholders) who were otherwise socially indistinguishable from the officials whom they displaced. Nowhere was there an influx of businessmen into positions of power.

I do have a few quibbles. Why list Louis Chevalier's “dangerous classes” among the

sources of opposition when Pinkney himself goes on to demonstrate that the revolutionary crowd (echoes of Rudé) comprised respectable artisans? Or why mention rural grain riots at all when they do not affect the course of events in 1830? And does not the account of replacing paving stones belong to gentle antiquarianism? But these are not serious criticisms.

In shifting to a consideration of this work as a model of synthesis for other American historians to follow I am troubled by what I perceive as lost opportunities. Pinkney's synthesis strikes me as unduly restrictive. If Pinkney's Law is to be seriously applied—and I think it should be—the context of historical problems should be as broad as possible and questions of comparison and significance raised without false modesty.

At least four such questions come to mind. First, while Pinkney treats the revolution of 1830 as a unique event he fails to consider it as a phenomenon, as belonging to a class of events. Just in the last fifteen years, modern revolution has been analyzed in numerous theoretical studies of varying quality, which, as Lawrence Stone has pointed out, we ignore at our peril. Second, granted that the administrative purges of 1830 failed to invest the bourgeoisie with state power, should our investigation stop there? I worry over Louis Philippe, an aristocrat to his finger tips, suddenly affecting a bourgeois life-style after 1830. Why did he if nothing had changed? Is it just possible that the social climate did alter even though the social and professional background of politicians and officials did not? Third, I remain perplexed by the constitutional issue that triggered the overthrow of Charles X. Why was it that this constitutional impasse over ministerial responsibility never recurred during the eighteen years of the July Monarchy? The question is central in assigning to the July revolution its proper place in French constitutional history. And though it unfairly transcends the time span of Pinkney's book the question will trouble our “American readers.” Fourth, I am puzzled by the birth, or rebirth, of popular republicanism in Paris following the revolution of 1830. If Parisian republicanism was so insignificant in July 1830 and the insurgents animated chiefly by hatred of the Bourbons, how do we account for the revolutionary republican challenge that

developed between 1831 and 1834? Did this republicanism grow out of the experience of 1830? If so, how and why? The question will not be dismissed by pointing out that December 31, 1830, marks Pinkney's chronological terminus.

By conventional standards *The French Revolution of 1830* is an excellent piece of scholarship, intelligently organized, elegantly written, and beautifully produced. As a pioneer effort to reorient American scholarship on France we may hope that it opens the way to even bolder and more searching reassessments of the French past.

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JEAN-PAUL ADAM. *Instauration de la politique des chemins de fer en France*. (Publications de l'Université de Rouen.) Paris: Presses Universitaires de France. 1972. Pp. 201. 35 fr.

This short monograph, concerned with the establishment of a general policy to govern railroad construction in France, focuses upon the role of the extraparlimentary commission, appointed in August 1839 and presided over by Jules Dufaure, the minister of public works. The recommendations of this commission subsequently provided the basis for the Railroad Law of 1842. The first question to be resolved by the commission was who would build the railroads: the state, private companies, or a combination of the two. Since the Chamber of Deputies had already rejected a government bill in 1838 providing for state construction of the major lines, this was not a politically feasible possibility. The commission recommended a joint effort, with the state acquiring the right of way and preparing the roadbed. Private companies were to lay the rails, provide the locomotives and rolling stock, and operate the lines. Mr. Adam concludes, wrongly in my view, that the choice was imposed because the total cost was beyond the resources of either the state or the private sector alone. A better explanation would be that the mixed system neatly balanced competing political and economic interests.

Among the many other questions taken up by the commission was how the state could provide additional aid to the companies. Outright grants

from the state were rejected, but the commission recommended that loans, purchase of shares, and guarantee of interest on shares issued by the companies could be used according to the circumstances. Mr. Adam concludes that of the three permissible means, the commission favored guaranteed interest on railroad shares, which influenced subsequent adoption of this method. Proposals by the commission for ameliorating the law for expropriating private property were legislated in 1841.

While older works have concentrated upon the parliamentary history of the Railroad Law of 1842, the chief merit of Mr. Adam's study is to describe the preparatory work of the commission. Although he adds some important details, the overall story is not altered significantly. Mr. Adam's self-imposed limitation of concentrating narrowly upon the work of the commission results in neglecting outside political and economic forces that contributed to shaping French railroad policy.

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ZEEV STERNHELL. *Maurice Barrès et le nationalisme français*. Preface by RAOUL GIRARDET. (Cahiers de la Fondation nationale des sciences politiques, 182.) Paris: Armand Colin. 1972. Pp. 395. 75 fr.

ROBERT SOUCY. *Fascism in France: The Case of Maurice Barrès*. Berkeley and Los Angeles: University of California Press. 1972. Pp. x, 350. \$15.75.

Both these works challenge the argument that fascist ideology had no significant "roots" in the French political tradition. Each author tries to trace such roots back to Maurice Barrès, whose appeal to young French nationalists before the First World War was surpassed only by that of Charles Maurras. Sternhell gives us an exhaustive intellectual biography of Barrès, describing the influence of other thinkers on him and the various stages of his ideological development. This is good old intellectual history modified to accommodate the vagaries of a self-dramatizing literary publicist. Sternhell seeks the ideas of Barrès in his journalistic writings in order to avoid Barrès's retrospective revisions of his own thought. Contrary to Sternhell, Soucy maintains that the source of Barrès's ideas is

most apparent in his novels and stresses the autobiographical character of the early ones. Soucy also sees the depiction of Barrès's youthful rebellion in *Under the Eyes of the Barbarians* (1888) and *A Free Man* (1889) as expressing a generation gap between authentic and idealistic youth and the phony and corrupt older generation. At the time of the Dreyfus affair Barrès bridged the gap by presenting racist nativism and integral nationalism to France's university youths as the best means for revitalizing their existence. After 1903 Barrès himself became less fascist and more conservative, but both Sternhell and Soucy insist that his fascist influence lived on into the postwar years.

In their documentation and interpretations of Barrès's role in pre-1914 French politics both authors are usually impeccable. Unaccountably, however, neither one mentions Frederic H. Seager's definitive study, *The Boulanger Affair: Political Crossroad of France, 1886-1889* (1969), which emphasizes the initial support for General Boulanger by lower-middle- and lower-class Frenchmen who felt unrepresented in the existing political system. This view is somewhat different from Sternhell's, according to which Boulanger's supporters on the extreme Left wanted to overcome the immobilism of the Opportunist ministries. The difference in interpretation is important for the origins of fascism, which both Sternhell and Soucy seek through Barrès. Their problem is that Barrès came to view Boulangism as an attempt to mobilize the masses through populist nationalism only after the movement had shifted to the right and was already disintegrating.

What is lacking in Sternhell's study is a feeling for Barrès as a person and a certain type of alienated intellectual. There are fairly obvious psychological and sociological dimensions to Barrès's turning from anarchic individualism (*culte du moi*) to nativist nationalism as a means for working out his self-image and his rootlessness in an uncongenial (the code word was "decadent") society. Barrès's behavior was an early example of this kind of projection. In the twentieth century numerous other literary publicists have sought salvation through collectivist political ideologies of both the Right and the Left. But neither Barrès nor most of these others ever resolved the paradox of elitist intel-

lectuals idealizing "the people" as a community while disdaining them as real-life individuals. Indeed, as Sternhell points out, beginning with *The Uprooted* (1897) Barrès adopted an openly patronizing attitude toward "the people."

Soucy provides some of the insights lacking in Sternhell. He says that the narcissism of Barrès in his twenties was partly an escape from the "barbarian" bullies of his childhood. Then he shows how Barrès, discovering that his escape from reality made him feel more insecure than before, found both the security and reality he longed for by integrating his *moi* into the folk soul. "*La terre et les morts* gave him something solid and lasting to identify with, erased his sense of alienation, and offered him the emotional reassurance that comes from being a member of the herd" (p. 106). This last conceit was probably more a pose than a sincere conviction. But Soucy is surely right when he ascribes Barrès's penchant for politics to a desire to test his ideas in the brutal combats of the real world and when he notes that Barrès was a good enough politician to modify his ideas to fit the reality he found there.

The weakness of these two books lies not in their scholarship but in their reasoning about relationships between Barrès's ideas at the end of the nineteenth century and the fascism of the 1930s. The observation that two successive things are similar does not prove that the later one has a direct connection with the earlier one. To assert such a connection without empirical evidence is a logical fallacy: *post hoc, ergo propter hoc*. And to use the similarities as the evidence is to argue in a circle. As both Soucy and Sternhell admit, the fascist Barrès of the turn of the century gave way to a conservative nationalist Barrès who never returned to his earlier position and whose fascism—unlike the integral nationalism of Maurras—was not perpetuated in any movement or by any disciples. Hence there is no proof of "roots" (a sloppy metaphor that should be banished from historical analysis). Terms like "precursor" or "harbinger" of fascism also beg the question of a direct connection; used with caution they can be suggestive, though they strain one's credulity when extended, as they have been by other historians, to Napoleon III, Bismarck, and Giolitti. Individuals and movements take what they want from the past, selectively, and often

use it in perverse ways. After all, Marx stood Hegel on his head.

Maurice Barrès was a bad man by humane, liberal standards, and he was the first to admit this. The same thing can be said of Friedrich Nietzsche, Ezra Pound, Filippo Tommaso Marinetti—or even Jean-Paul Sartre in his recent “Maoist” stance. Nevertheless, in their zeal to prove that in 1900 Barrès’s ideas were closer to fascism than to traditional conservatism, Pétainism, or Gaullism, Sternhell and Soucy may unwittingly obscure the real horrors of fascist practices in the minds of some younger readers for whom all these things are only history.

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JEAN BOUSQUET-MÉLOU. *Louis Barthou et la circonscription d'Oloron (1889–1914)*. Preface by GEORGES DUPEUX. (Bibliothèque de l'Institut d'Études Politiques de Bordeaux, Centre d'Étude et de Recherche sur la Vie locale. Series Vie locale, 3.) [Paris:] Pedone. 1972. Pp. 268. 35 fr.

Louis Barthou is known to most people at the moment of his death in 1934 when, in the process of mounting a diplomatic coalition against Hitler, he was assassinated along with King Alexander of Yugoslavia. Very little has been written about his life before that. In this meticulous work Jean Bousquet-Mélou analyzes the complex relationship between Barthou, the Basses-Pyrénées politician, and his “electoral fief,” Oloron-Sainte-Marie, during the quarter century before World War I. Barthou’s years as deputy from 1889 to 1914 correspond to a logical electoral unit in French history because the *scrutin d'arrondissement* was reintroduced in 1889 to undermine the political appeal of General Boulanger’s followers, and this voting system remained in force through the election of 1914. Although Barthou’s political career did not end until 1934, this early period clearly constitutes a self-contained unit.

The author demonstrates that Barthou was a moderate by temperament. His moderate position was endorsed by a majority of the electors in his mixed rural-urban legislative district, a convergence that proved fortuitous as he was elected to the Chamber of Deputies seven times. The first election was very close; the twenty-seven-year-old blacksmith’s grandson encoun-

tered stiff opposition from his conservative opponent, the scion of a “notable” local family. Barthou received a majority of only 300 out of 13,763 votes. The only other difficult election in the first half of the statesman’s career came in 1906 when he, as a consistent anticlerical, lost substantial Catholic support when the separation of Church and state became an emotional political issue.

One of the most significant trends in recent French historiography has been an emphasis on regional case studies. Local centers of historical research, such as the Institut d'Études Politiques de Bordeaux, have particularly encouraged such work. And these detailed political treatments furnish essential building blocks in reconstructing the history of France. This monograph is particularly valuable to students of the Third Republic concerned with the impact of local issues on national politics. Clearly a major figure in this period, Barthou was a sophisticated politician whose talents have too long been overlooked. Bousquet-Mélou shows just how skillful he was, both in the capital and in his home district. In so doing the author presents a careful analysis of the relationship between regional pressure groups and Oloron’s permanent deputy in Paris.

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FÉLIX PONTEIL. *Les bourgeois et la démocratie sociale, 1914–1968*. (Collection “L'évolution de l'humanité,” number 32.) [Paris:] Éditions Albin Michel. 1971. Pp. 560. 12 fr.

In his foreword M. Ponteil confesses that his work lies somewhere in the gray zone between journalism and history and chooses to leave the verdict to his readers. Companion to *Les classes bourgeoises et l'avènement de la démocratie (1815–1914)*, published in 1968, this volume figures in the series “L'évolution de l'humanité,” and, given its title, it is striking that more than eighty per cent of its pages are devoted to France while the remainder are given to perfunctory and often superficial analyses of the Russian Revolution, the Weimar Republic, and England under the Labour party, 1945–51. Sometimes elegant, always intelligent, this is a study of the evolution of French politics and

society since World War I by a determined partisan of social progress. Unfortunately it is uneven in its treatment of key problems and marred by the author's unyielding addiction to hyperbole, overtly polemical characterizations (Léon Blum, "afraid of reality," or, in an approving quotation from an undisclosed source, denunciation of a Socialist leader as that "heavy-jowled equivocator of European Social Democracy"), and by an intransigent insistence upon right versus wrong, upon history in black and white.

Briefly put, M. Ponteil argues that despite a certain progress social democracy is far from realization, notwithstanding fleeting opportunities and working-class heroics, notably in the aftermath of World War I, in the Popular Front period—dismissed as a "straw fire"—and in the violence and ambiguity of 1968. The two camps, bourgeoisie and workers, were frequently in open combat, both conscious of their class solidarity, although the lineup has been altered somewhat by the children of the bourgeoisie who have thrown in their lot with the workers. The barriers to socialism and genuine democracy, argues M. Ponteil, are intellectuals whose very methods sow dissension and dissolution, politicians for whom lying is a way of life, changes in classes, lack of cohesion on the Left, the instinctive conservatism of the French, and, most especially, Socialist politicians. All of this may provoke controversy and further speculation but certainly little surprise, and it may lead readers to conclude that this is not formal historical interpretation or even a synthetic textbook but a tract, and a very long one at that. Despite the setbacks, the pusillanimous behavior of working-class leaders, and the evolution of capitalism M. Ponteil believes the future cannot fail to belong to socialism. The apparent convalescence of capitalism since 1945 is but a temporary remission of its terminal illness, and, like Jean Jaurès, M. Ponteil is convinced that social democracy, by slow progression, will be the fulfillment of the emancipatory process begun in the Revolution.

Whatever the interest evoked by these arguments it has to be said that the text is ill-organized and ill-disciplined. Accusation and compartmentalization often take precedence over careful analysis. If much attention is given to the crucial years of the Third Republic, 1919–

40, the familiar themes of decadence and decline are reaffirmed without doubt as to their truth, and in speaking of domestic crises little or no mention is made of the impact of foreign affairs in the 1930s. Specialists will learn little that is new from this volume, but if the reader is not wholly dismayed by the ease with which M. Ponteil classifies and characterizes entire classes, he will gain insights into the historical methodology of an articulate advocate of social democracy.

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R. WILLIAM RAUCH, JR. *Politics and Belief in Contemporary France: Emmanuel Mounier and Christian Democracy, 1932–1950*. The Hague: Martinus Nijhoff. 1972. Pp. 351. 47.25 gls.

In *Le Monde* of April 6, 1973, André Fontaine identified the task of those who lost in the March elections in France as one of continuing to challenge the "established disorder." When Fontaine reminded his readers that the "established disorder" was Mounier's expression, he testified to the continuing echo of Mounier's voice in the political consciousness of his society. Mounier will also be remembered for having founded the important journal *Esprit*, but beyond this major accomplishment his achievement remains debatable and precarious. Roy Pierce in his judicious *Contemporary French Political Thought* (1966) accurately observed that Mounier "literally erected ambivalence into a philosophical principle" (p. 84). William Rauch's meticulous account of Mounier's thought and action proposes that Mounier's example made possible the pluralism of contemporary French Catholicism.

This study, conceived and executed in the style of Adrien Dansette, is an admiring, but not exactly hagiographic presentation of Mounier. He is characterized as Péguy's worthy and noble intellectual heir and praised endlessly for the "profundity" of his "spirituality." Yet it is impossible for those uninitiated into Mounier's ascetical politics to determine exactly the substance of his messages. Mounier intended to be a Christian witness in the modern world. But what is striking, if unintentional in Rauch's careful analysis, is the unmistakable evidence that Mounier remained

a confessional moralist consumed by the debates within the Catholic community of France.

Professor Rauch persuasively argues that Mounier's criticism of the doctrinal pride and elitism of the Christian democrats devastatingly exposed the limits and prejudices of this political camp. The historian would like to know, however, in a more convincing fashion, why Mounier's piety demanded his uncertain judgment of February 6, 1934, his hesitations concerning the Popular Front, his initial confusion with regard to Vichy, and the trembling of his hand when he offered it to communists. Jacques Maritain, in his most reactionary *Le Paysan de la Garonne* (1966), claimed that Mounier invented the expression "personnaliste et communautaire," and Maritain further added that it became through Mounier a "tarte à la crème" for Catholic thought and rhetoric (p. 82). Rauch suggests that Mounier brought the bread of life to his generation. Rauch's valuable study will allow his readers to decide the merit of this interpretation or that of Mounier's *cher maître*, the peasant philosopher from the Garonne.

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JUAN FRIEDE and BENJAMIN KEEN, editors. *Bartolomé de Las Casas in History: Toward an Understanding of the Man and His Work*. DeKalb: Northern Illinois University Press. 1971. Pp. xiii, 632. \$20.00.

Almost five hundred years after his birth in 1474 the figure of Bartolomé de Las Casas continues to fascinate and infuriate people of every degree. A man capable of unleashing as many passions (e.g., the attack by Menéndez Pidal) as the fiery bishop of Chiapa must a priori be a subject of considerable historical interest. This interest is heightened by the fact that the problems he confronted and fought—racism, colonialism, the conflict of cultures, the justice of war, social justice, ethnography—are with us yet. The fact that he found his way toward solutions and fought for over half a century in behalf of these solutions, and so in behalf of the oppressed and exploited everywhere, makes of Las Casas a man who has as much to say to our age as he had to his own. As Manuel Giménez Fernández has observed, the

men of Vatican II were his spiritual brothers.

All the more remarkable then, especially in view of the deluge of Lascasian literature in this century alone, is the fact that so much about the man remains to be studied. Hence this handsome volume is a welcome addition to the already abundant Las Casas literature. It serves as a fine introduction for the scholar who is not well acquainted with the subject and as a fine survey for the one who is. Its twelve essays embrace the high points of Las Casas's career and thought and do so with remarkable thoroughness and balance. The various offerings maintain a consistently high standard and are developed with logic and continuity.

The first two essays would by themselves justify the book. These are Benjamin Keen's historiographic survey ("Approaches to Las Casas, 1535-1970") and Giménez Fernández's moving if occasionally cryptic survey of Las Casas's life ("Fray Bartolomé de Las Casas: A Biographical Sketch"). Juan Friede ("Las Casas and Indigenism in the Sixteenth Century") follows his career from preacher to politician to activist and traces the radicalization of his thought. This essay also contains a good summary of the Spanish colonial experience in the Canaries and of the effects of the American reality on Las Casas's thought. It can be argued, however, that he overemphasizes the decline of the Lascasian school after and even before the death of the bishop. More recent researches on such events as the Third Mexican Council (1585) and some of the later Mexican bishops indicates that the movement still had vitality around the end of the century.

In the essays dealing with the ideology of Las Casas that of Venancio D. Carro, O.P. ("The Spanish Theological-Juridical Renaissance and the Ideology of Bartolomé de Las Casas") is to me the least satisfying in the volume. It is marred by excessive praise for the Dominican order. Not only is such glorification distasteful but it ignores the historical reality that there were also non-Dominicans who were on the side of the angels. Carro is also on questionable ground when he uses Vitoria as the yardstick to measure the correctness of Las Casas's opinions. Angel Losada ("The Controversy between Sepúlveda and Las Casas in the Junta of Valladolid") summarizes the material to be

found in the still unpublished *Apologia*. Father Manuel Martínez, O.P. ("Las Casas on the Conquest of America") gives a more balanced interpretation than does Carro, especially about Las Casas's ideological relationship to Vitoria.

The third section, "Las Casas in America," begins with a fine essay by Marcel Bataillon ("The *Clérigo* Casas, Colonist and Colonial Reformer") that emphasizes two early, formative influences: his experience as a diocesan priest and the residual effects of the colonial outlook. Father Benno Biermann, O.S.B. ("Bartolomé de Las Casas and Verapaz") gives a good account of this gallant but eventually unsuccessful experiment. The final section, "The Heritage of Las Casas," deals mostly with literary and historiographic material, with essays by Juan Comas ("Historical Reality and the Detractors of Father Las Casas"), V. Afanasiev ("The Literary Heritage of Bartolomé de Las Casas"), and Raymond Marcus ("Las Casas in Literature"). The latter has also contributed the excellent bibliography.

The format of the book is attractive and the illustrations well chosen. Remarkably for such an extensive work typographical and other errors are almost entirely absent. It should be pointed out, however, that "Castilian" on page 12 should clearly be "Catalan" and that "zeal for God's house" (p. 337) does not refer to the Dominican convent but is a citation of Psalm 68 in the Vulgate numbering.

This volume is a fitting memorial not only to Las Casas but also to the late Manuel Giménez Fernández, to whom it is dedicated. Like Las Casas he was a long-time Christian critic of his own government, and his concluding words are an apt tribute to his own life as well as that of the bishop of Chiapa: "Las Casas is the most admirable of the sons of Seville; and around his doctrine, properly applied to present-day conditions, can rally all who are disillusioned with anarchistic individualism, with totalitarianism which degrades the human personality, and with the servile legalism that grovels before Caesar."

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PAULINE CROFT. *The Spanish Company*. (London Record Society Publications, volume 9.) [London:] the Society. 1973. Pp. lii, 143. £4.50.

Historians have a success syndrome that causes failures to be understudied, though failures—whether politicians, policies, enterprises, or whatever—are the essential obverse of successes and often play an important role themselves. The present volume helps right that imbalance, for the Spanish Company (with origins traceable to 1517 or earlier) was a perennial failure throughout its existence.

The book contains a 45-page introduction, equally divided between "The First Spanish Company, 1530–1585" and "The Revival of the Company, 1604–1606," plus 124 pages of documents regarding the final period: the company's *Register Book*, 1604–06 (73 pages); the *Book of Oaths, Acts and Ordinances* (21 pages); the 1605 charter (19 pages); and five additional documents (11 pages) referred to in the *Register Book*. (Length matters particularly in this sort of volume: in a typical secondary work, pagination would be almost double the tall pages and small type here.) Editing is excellent throughout, including an 18-page index (for 169 pages of text).

The book is useful on three levels. Croft's admirable introduction will reward anyone interested in economic or business history or interested in the period in general. The documents contain much hard data for the researcher. And, in between, from these documents the attentive reader will gain considerable insight into fundamental problems not broached in the introduction. For example, the need for six consulates ringing the peninsula (pp. 50–51), reflecting difficult internal communications, contrasts sharply with the typical chartered company's more effective single entrepôt with access to its whole market area. A merchant's servant arrested by the Inquisition for religious utterances, released, then re-arrested nine months later "for the same words so spoken" (p. 119) suggests that the English brought some of these troubles on themselves.

These documents originate in England and deal mainly with events in London, not in the market area; though Croft's introduction goes well beyond these documents, it is, understandably, similarly limited. The full story naturally requires use of the English ambassadors' reports and Spanish conciliar and other sources, but work is already afoot in

those, and meanwhile we have here a very useful volume.

This ninth annual volume of the London Record Society has, like the eighth, ramifications far beyond the bounds of municipal history. Such an unparochial publication policy bodes well for the series.

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LEON TROTSKY. *The Spanish Revolution (1931-39)*. Introduction by LES EVANS. New York: Pathfinder Press. 1973. Pp. xix, 22-446. Cloth \$10.00, paper \$3.95.

PIERRE BROUÉ. *La révolution espagnole (1931-1939)*. (Questions d'histoire.) [Paris:] Flammarion. 1973. Pp. 190.

HERBERT L. MATTHEWS. *Half of Spain Died: A Reappraisal of the Spanish Civil War*. New York: Charles Scribner's Sons. 1973. Pp. ix, 276. \$10.00.

WILLIAM E. WATTERS. *An International Affair: Non-Intervention in the Spanish Civil War, 1936-1939*. (Exposition-University Book.) New York: Exposition Press. 1971. Pp. 423. \$12.00.

The most interesting of these four works is the collection of Trotsky's writings. From the moment that the king dismissed Primo de Rivera in January 1930 the exiled Russian leader had eagerly followed Spanish developments. He knew that the Communist party was very small and faction-ridden. He hoped to win its best militants for a "Bolshevik-Leninist" rather than a Stalinist policy, and he was optimistic about the combative quality of the Spanish working class. He did not think that the parliamentary stage of revolution could be skipped, but he pictured it as being very brief. He advised his friends not to endorse, much less participate, in any bourgeois government, and he urged them to give ideological and organizational leadership to the uncoordinated strikes taking place in 1931-32. From early 1933 until October 1934 Trotsky was fully occupied with the German situation and with his own efforts—he had moved from Turkey to France in July 1933—to found the Fourth International. Writing in November 1934 he attributed the defeat of the Asturian rising to the "unprincipled political reformism" of the Spanish Socialist party, which, like the Mensheviks and Social Revolutionaries in Russia,

had shared power with bourgeois parties and thereby prevented the development of the proletarian revolution. He believed that Socialist collaboration with Azaña had strengthened anarchosyndicalism on the Left and allowed "social Catholic demagoguery" (i.e., the CEDA of Gil Robles) to exploit the disappointment of the rural masses at the absence of any radical social reform. He was also bitterly critical when his former disciple Andrés Nin joined forces with the "muddled" and numerically insignificant Workers and Peasants Bloc of Joaquín Maurín to found the POUM. If Nin and his followers had been true Bolshevik-Leninists they would have joined the Caballero Socialists in order to prevent the revolutionary wing of the party from making a close alliance with the Stalinists, as happened when the Socialist and Communist youth movements were merged in April 1936.

Trotsky viewed the Popular Front not as an instrument of revolution, but as a means whereby the frightened bourgeoisie obtained Socialist and Communist cooperation to prevent revolution. During the Civil War he called on his followers to be the "best fighters" against the fascists, but he absolutely rejected the Communist and right-wing Socialist slogans about winning the war before thinking of revolution. On the contrary he argued that the masses, and especially the peasants, would have no reason to prefer the Popular Front to Franco unless there was going to be a fundamental land reform. The way to win the war (as in the Russia of 1917) was to urge the workers to seize the factories and the peasants to take the land. "The fascist army could not resist the influence of such a program for 24 hours," he wrote on July 30, 1936. And in April and May 1937 he reiterated that without the proletarian revolution even a possible victory for the Republican armies would only be a roundabout path to fascism. His exposure of vacillations and contradictions often contain delightful flashes of humor as when he writes, concerning *La Batalla's* fumbling endorsement of the Popular Front: "One cannot say on Monday that the League of Nations is a band of brigands; on Tuesday urge the voters to vote for the program of the League of Nations; on Wednesday explain that it was only a question of electoral action, and that today one has to resume one's

own program." But Trotsky, for all his brilliance, fundamentally misjudged the Spanish context. Constantly drawing parallels to the October Revolution, he was unable to recognize that the Spain of 1931, and of 1936, had not (in contrast to the Russia of 1917) been ravaged by three years of war, typhus, and transportation breakdown; nor did he realize that the army, the Church, and the middle classes possessed corporate strength far greater than that of their Russian counterparts. For him all was a question of boldness, but he may have been right in his psychological judgment that Nin did not really want to lead a revolution. The book is well indexed and contains very useful thumbnail biographies of the many persons mentioned in the documents.

The first hundred pages of the Broué volume are devoted to a brief analysis of the Spanish Left in the 1930s. The interpretation is very similar to that which he gave in Broué and Témime, *La révolution et la guerre d'Espagne*, but the discussion concentrates more on ideas and political organizations than upon narration. This section is followed by fifty pages of documents well chosen to illustrate the evolution of the Socialist and Communist parties, the CNT, the FAI and the POUM, and the incursion of Soviet politics in Spain. Perhaps the most valuable feature of the book for scholars is the twenty-page "state of the question" essay. No other readily available volume identifies and places so many political leaders of the Spanish Left, and no other scholar has so successfully clarified their significant ideological, political, and personal differences. The footnotes contain unique bibliographical references for the immense periodical literature on the subject.

Herbert Matthews was one of a handful of truly superior journalists who both reported the Civil War vividly and interpreted it with great insight. The present book, however, has a curiously disjointed character. It is as if the author had reviewed his own dispatches, recalled his emotions and interpretations of the time, read Gerald Brenan, Burnett Bolloten, Hugh Thomas, Stanley Payne, and Gabriel Jackson, decided where he agreed and where he disagreed with each of them, and then sat down at the typewriter. The factual outlines, seen from a generally pro-Loyalist perspective, but

without whitewash, are there. Nevertheless, his new reading, and the history of the last three decades, do not seem significantly to have affected his thinking. Thus the book has an inert, anachronistic quality, and it is likely to disappoint those who have read such earlier Matthews works as *The Education of a Correspondent* (1946) and *The Yoke and the Arrows* (1957). Mr. Watters's book will not be of great interest to historians. It is superficial diplomatic history, based principally on newspaper sources, parliamentary records, and published diplomatic papers. He is uncritical of these sources, and he seems to be almost completely unaware of the hundreds of books and articles that could have contributed to a really scholarly synthesis concerning the international aspects of the Civil War.

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A. H. DE OLIVEIRA MARQUES. *History of Portugal*. Volume 1, *From Lusitania to Empire*; volume 2, *From Empire to Corporate State*. New York: Columbia University Press. 1972. Pp. viii, 507; 303. \$15.00 each.

With the publication of this two-volume study, Oliveira Marques has provided the English reader with the most significant general history of Portugal and her empire to date. The work is a decided improvement on its predecessors. The author, who is already noted for monographic studies in the medieval period and, most recently, for a history of the First Republic, has brought together the wide range of his research to present a balanced narrative. The introduction and chapters 1 and 2 compress an array of factual information into a discussion of Portugal's origins as a state and her medieval development. Chapters 3 and 4 are allotted to the discoveries, rise, and collapse of the first overseas empire, and the impact of these events upon Portugal during the years 1400–1700. Three chapters treat the seventeenth and eighteenth centuries, one on Portugal and the remaining two on Brazil and the rest of her tridimensional empire. The second volume, encompassing the period from the nadir of Portugal's power in the 1820s until the present, is almost equally divided between the nation's own gradual internal evolution

toward republicanism and the New State and the creation of a third empire in Africa.

The tone of this work is anything but impartial. In his quest for broad underlying trends and long-range movements to describe Portugal's development, the author presents a tour de force of interpretive history. Three particular examples are outstanding. Oliveira Marques thrusts himself into the long-standing debate over the creation of Portugal from a cultural uniqueness in the dim past versus political events in eleventh-century Iberia. He opts for the former and shows convincingly how Portugal emerged from centuries of fusion between two distinct cultural regions in the western peninsula—the first is an area north of the Douro River containing dominant Celtic-Suevian-Christian influences, while the second region lies south of the Tagus, with its deep African-Muslim traditions. The *reconquista* is viewed as a bringing together of these divergent cultures to create a national identity.

It is refreshing to note that the author rejects the notion of an "artificial seignorial non-feudal Portugal" that has been the former trend among most historians. Utilizing recent scholarship in this field, he argues convincingly for a Portuguese feudal society with unique elements not found elsewhere in European feudalism. In particular, emphasis is laid upon Mozarabic and Muslim feudal society, which is derived from the southern portion of the kingdom. In his own words, "once the idea of a monolithic and geographically restricted feudalism is put aside, the interpretation of medieval and early modern Portugal as a feudal state ceases to be a riddle."

The third major theme the author develops concerns the close economic relationship between Portugal and her colonial possessions. He traces the development of a new colonial policy in the late nineteenth and twentieth centuries and seeks to understand colonial growth as being inextricably linked with Portugal itself. The African colonies of Angola and Mozambique, in particular, are visualized as a "magnified replica of the history of Portugal, with all of her slow economic development, defects in social structure, and cultural backwardness."

Oliveira Marques interprets Salazar's New State as a totalitarian regime, despite its façade

of representative democracy. Marques's treatment of Portugal's economic and political development since 1926 is particularly valuable to those readers who have not seen Hugh Kay's *Salazar and Modern Portugal* (1970), which, incidentally, is included in the bibliography.

Perhaps the least successful portion of the work is the treatment of the constitutional monarchy (chapter 10). Though every page is filled with statistical data and a recounting of the labyrinthine political and economic maneuvers, the author never quite comes to grips with the period as a whole. His discussion remains somewhat fragmented and difficult to follow. Perhaps one of the reasons for this is the placement of the reciting of chronological events at the end rather than at the commencement of the chapter. Another flaw is the misplacement of maps (pp. 63, 112).

Despite these minor shortcomings, much merit remains. It is precisely Oliveira Marques's persistent concentration on the social and economic, rather than political, aspects of his history that makes it a welcome study for students seeking a significant introduction to Portuguese history. Even the experienced scholar will find something of value in the interpretations presented here, although he may not agree on all points. Also of value is the annotated bibliography at the end of each volume, which presents the most recent research, including works by Magalhães Godinho, Paulo Merêa, Russell-Wood, and the author himself.

JOHN VOGT
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JORMA KALELA. *Grannar på skilda vägar: Det finländsk-svenska samarbetet i den finländska och svenska utrikespolitiken, 1921–1923* [Neighbors on Separate Roads: Finnish-Swedish Cooperation in the Finnish and Swedish Foreign Policies, 1921–1923]. (Historiallisia tutkimuksia, 84.) Helsingfors: Suomen Historiallinen Seura. 1971. Pp. 313.

Finnish scholars have been relatively slow in coming to grips with their country's history during the period of independence, but recently there have appeared a number of book-length studies in particular about several aspects of the international relations of Finland. Jorma

Kalela's work falls into this last category. It is a study about Finland's relations with Sweden within a two-year period. Kalela has sought to answer the question of why these two neighboring states did not establish closer connections and cooperation with one another. His basic conclusion is that their conceptions of their basic self-interests were too different to allow for close military or diplomatic cooperation. According to Kalela this fundamental fact rather than any single issue, like the dispute over the Åland Islands or the language and nationality conflict in Finland between the Swedish-speaking minority and the Finnish-speaking majority, kept the two states politically apart from one another.

In order to solve his research problem Kalela thought it necessary to rely heavily for his theoretical framework, interpretation, and terminology on such American political scientists as Richard Snyder, Harold Sprout, Michael Brecher, and Herbert Kelman. His as such commendable effort to define and be precise in terminology has, however, led him to excessive use of social-science jargon in discussing relatively simple and clear-cut historical issues with the result at times of obscuring rather than clarifying them. The clarity of the study also suffers from insufficient integration of the historical material with the theoretical framework, in spite of efforts to make it fit to the extent of downplaying or bypassing evidence that does not seem to support the adopted theoretical framework and hypothesis. In general the author ignores or underestimates the influence of emotional or irrational factors, the plain logic of events or the dynamics of policies as reactions to one another. Just to mention one example, he neglects to assess or analyze the impact of Sweden's refusal to join the "Whites" in the Finnish civil war.

The work adds little in the way of original contribution to the theoretical discussion of the nature of international relations, and, because of its narrowly circumscribed subject matter, it does not either provide much in the way of significant new historical information. It may familiarize some Scandinavian readers with a few standard American social-science concepts that they perhaps had not been confronted with previously. Its main value seems to lie in the author's effort to give balanced

treatment and coverage to both sides when discussing the relations between the two states.

On the whole the bulk of the work is extensively documented, but there are some odd omissions in the bibliography and a few factual mistakes. But Kalela strives to be objective and unbiased in discussing the policies of the two countries, and by and large he succeeds in this effort.

PEKKA KALEVI HAMALAINEN
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Madison*

H. C. ERIK MIDELFORT. *Witch Hunting in South-western Germany, 1562-1684: The Social and Intellectual Foundations*. Stanford: Stanford University Press. 1972. Pp. viii, 306. \$11.50.

In his examination of witch-hunting in south-western Germany Professor Midelfort makes a solid contribution to our understanding of this strange phenomenon of the sixteenth and seventeenth centuries. He set himself the task of testing previous explanations of witchcraft and also of addressing himself to new questions raised by legal historians, anthropologists, and psychoanalysts by studying local, legal, and archival evidence within a limited area, that comprising Baden and Württemberg with their many different political and ecclesiastical jurisdictions. To make the mass of materials manageable, he examined in detail only the large witch-hunts, which he arbitrarily defined as those resulting in twenty or more executions a year.

Avoiding the sensational, Midelfort considers the three crucial elements involved in witchcraft, namely, the pact with the devil, demonic power, and divine permission. He points out that there was no clear orthodox theory concerning the essence of the mania but a combination of theories based on current legal practice, scholastic reasoning, and practices of heretics. Although virtually no one doubted the power of the devil or the presence of witches, men differed with respect to the significance they placed on the witch's pact with the devil, the difference between harmful and harmless magic, the ability of witches to cause physical harm, and the providential role in storms and other disasters.

The most important conclusion of this study

is that the large witch-hunts of southwestern Germany came to an end, not because of intellectual enlightenment but because the witch-hunters had reached a crisis of confidence in their ability to identify witches. Among other important conclusions are the following: that greed played little or no role; that Europe in the Reformation era was too far advanced to warrant the application of findings in primitive societies; that there is no evidence to support the contention that Catholicism was more severe on witches than Protestantism, even though there were more large trials in Catholic lands; that the stereotype of the old woman as witch broke down; that no one class provided the victims; and that the large trials served no discernible social function. The book contains a detailed bibliography, maps, illustrations, and numerous tables of data concerning the trials.

HAROLD J. GRIMM
Ohio State University

HERBERT SCHOTTELIUS and WILHELM DEIST, editors. *Marine und Marinepolitik im kaiserlichen Deutschland, 1871-1914*. Düsseldorf: Droste Verlag. 1972. Pp. 328. DM 38.

The Imperial navy, even as contemporaries recognized, represented a microcosm of German society. For the most part, however, historians of the 1897-1914 period have treated the building of a navy as the visible sign of Wilhelmian Germany's *Griff nach der Weltmacht* while neglecting the economic, social, and political implications of the fleet-building program. With the centralization of the military archives in Freiburg, scholars were able to study in detail an institution that was of pivotal importance for both the foreign and domestic politics of Imperial Germany. This volume, the record of the April 1972 Kirchzarten conference on the theme "Navy and Naval Politics in Imperial Germany 1871-1914," demonstrates the breadth and direction of the new research and the nature of the issues involved in the debate over the building of the German fleet. In his summary essay, Friedrich Forstmeier adroitly presents the historical verdict in six key areas: the justification for the building of a fleet; the tasks and aims of the German fleet; the development of German-English relations in the period of competition; the impact of the *Dreadnought* on the naval race; the domestic political

implications of the Tirpitz plan; and the importance of Tirpitz's personality for the program and the development of the navy.

The most important contribution of the thirteen German and foreign scholars is Volker Berghahn's "Der Tirpitz-Plan und die Krisis des preussisch-deutschen Herrschaftssystems." Berghahn describes the building of the German navy as the result of a deliberate "crisis strategy" in order to maintain the status quo of Imperial Germany's ruling elite against the Reichstag and the growing threat of socialism. At the same time, the Imperial navy would force England to make way for Germany's overseas expansion.

The thesis of a duality in domestic and foreign politics, explored more fully in Berghahn's work on the origins and fall of the Tirpitz plan (*Der Tirpitz-Plan. Genesis und Verfall einer innenpolitischen Krisenstrategie unter Wilhelm II* [1971]), constitutes a central theme of the contributors of this volume and establishes a valuable framework for further research. Peter-Christian Witt's study of Reich finances and armaments policy reveals the government's unsuccessful attempts to replace the antiquated tax structure, which had been strained considerably by the increased costs in shipbuilding after the *Dreadnought* "break-through." The financial needs of the military, especially the navy, threatened the system-stabilizing *Sammlungspolitik* and, in the end, caused the government to lose the support of those very political, social, and economic groups whose interests it was trying to preserve. With the Morocco crisis, the dilemma of German *Weltpolitik* became obvious and the armament needs of the army received priority. This return to Germany's more traditional Continental policy in 1911, as corroborated by Harmut Pogge v. Strandmann's essay on the role of the national leagues, clearly demonstrated that building of a navy had failed to fulfill either Germany's domestic or foreign policy goals. And discussions of the "Prussianizing" of the naval officer corps (Holger Herwig) or the problems of German naval strategy (for example, Paul Kennedy and Edward Wegener) reflect just how completely the hopes (and claims) of the navy's supporters were shattered in the last years before 1914.

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FRITZ T. EPSTEIN. *Germany and the East: Selected Essays*. Edited, with an introduction, by ROBERT F. BYRNES. Bloomington: Indiana University Press. 1973. Pp. xix, \$6.95.

Fritz Epstein is widely known for his bibliographical expertise, and countless researchers are indebted to him for sound advice and generous assistance. His historical essays, scattered through various German journals, will be known only to a few specialists. Some of these essays—originally published (with one exception) between 1954 and 1966 and now slightly revised—have been translated and brought together as a tribute to Epstein by his former colleagues at the Russian and East European Institute at Indiana University. Robert F. Byrnes, who signs as editor, has contributed an affectionate introduction, blending biographical detail with an appreciation of Epstein's scholarly interests and accomplishments.

The nine essays in this collection are grouped around the theme set forth in the title. Five deal with the ways in which German politicians and historians have conceived of Eastern Europe and Russia and how these conceptions shaped historical thought, political analysis, and, especially, foreign policy. To elucidate, Epstein focuses on Germany's *Ostpolitik* during World War I, which, as it culminated at Brest-Litovsk, aimed at controlling and exploiting the East. Epstein first traces the intellectual roots of this wartime policy back into the mid-nineteenth century, to the concept of *Mittleuropa*, which was as much defensive as it was offensive. Then, in two long review essays of the work of Fritz Fischer and of some of his critics, Epstein examines the dissemination and application of this policy, assessing both its impact and its importance. Finally, in pieces on Otto Hoetzsch and Friedrich Meinecke, two conservative historians, he presents a contemporary critique of the policy. Taken together, these essays provide perspectives on Germany's relationship with her Eastern neighbors, which are as relevant today as they were sixty years ago.

Two of the remaining essays discuss Soviet Russia and the Versailles Treaty. In "Russia and the League of Nations" Epstein reconstructs the young League's attitude toward the Soviet Union. Soviet animosity toward the League, it would appear, arose not only because Western statesmen and White Russian generals saw the League as an instrument to counter

(or destroy) bolshevism. It grew also because the possibility of League mandates over Russian territories, proposed at the Peace Conference and promoted by representatives of various Russian nationalities, challenged the Soviet commitment to a unified state. In "The Question of Polish Reparation Claims, 1919-1922" Epstein examines Russia's (and Poland's) rights to reparations under the peace treaty, disentangling neatly the juridical and political aspects of these rights. There is also an essay on Soviet educational policy (dating from 1932) and one comparing the selection and publication of Germany's diplomatic documents after the two wars—the *Grosse Politik* and the *Documents on German Foreign Policy*.

Epstein's strength lies in accumulating evidence and reporting his findings. Each essay is carefully constructed and documented, drawing on a wide range of archival sources and recondite printed material and on a full knowledge of the secondary literature. The essays are monuments to great learning, to precision and attention to detail. They have suffered little from the passage of time.

CHRISTOPH M. KIMMICH
Columbia University

HAGEN SCHULZE, editor. *Das Kabinett Scheidemann: 13. Februar bis 20. Juni 1919*. (Akten der Reichskanzlei: Weimarer Republik.) Boppard am Rhein: Harald Boldt Verlag. 1971. Pp. lxxvii, 554.

MARTIN VOGT, editor. *Das Kabinett Müller I.: 27. März bis 21. Juni 1920*. (Akten der Reichskanzlei: Weimarer Republik.) Boppard am Rhein: Harald Boldt Verlag. 1971. Pp. lxxi, 375.

PETER WULF, editor. *Das Kabinett Fehrenbach: 25. Juni 1920 bis 4. Mai 1921*. (Akten der Reichskanzlei: Weimarer Republik.) Boppard am Rhein: Harald Boldt Verlag. 1972. Pp. lxxx, 720.

MARTIN VOGT, editor. *Das Kabinett Müller II.: 28. Juni 1928 bis 27. März 1930*. Volume 1, *Juni 1928 bis Juli 1929; Dokumente Nr. 1 bis 256*; volume 2, *August 1929 bis März 1930; Dokumente Nr. 257 bis 489*. (Akten der Reichskanzlei: Weimarer Republik.) Boppard am Rhein: Harald Boldt Verlag. 1970. Pp. lxxxviii, 835; v, 837-1682.

These volumes are part of an ambitious documentation project sponsored jointly by the Bavarian Academy of Sciences and the West

German Bundesarchiv, with Karl Dietrich Erdmann and Wolfgang Mommsen serving as general editors. The aim is to publish selected documents from the files of the Reich Chancellery for each of the cabinets of the Weimar Republic. One previous volume, dealing with the chancellorship of Wilhelm Cuno (November 1922 to August 1923) appeared in 1968 (*AHR*, 75 [1970]: 1150). After completion of the Weimar series, the editors plan to add additional volumes containing chancellery documents from the Third Reich. When coupled with the volumes on the period from October 1918 to February 1919, prepared under the auspices of the Commission for the History of Parliamentarism and Political Parties in Bonn, the Weimar and Third Reich projects will eventually provide a voluminous public record of central governmental authority in Germany between 1918 and 1945. This will be an achievement unparalleled in any other major country for such a recent period of history.

Although these five volumes make available in print more than a thousand documents, they contain no surprising revelations, since the full collection of Reich Chancellery files has been accessible to researchers at the Bundesarchiv for more than a decade. Part of the collection, mainly the minutes of the cabinet meetings and attached documents, was issued as a microfilm publication of the National Archives of the United States nearly twenty years ago and has similarly been used by many scholars. The value of these printed volumes lies in making possible a wider dissemination of the documents and in ordering them in such a manner as to maximize their intelligibility. The latter effect is achieved by the fortunate choice of a chronological system of organization that juxtaposes documents on concurrent developments rather than attempting to group them topically. This accords the researcher an opportunity to grasp the concatenation of events by reading seriatim documents that are scattered about in the archival collection, filed away under a variety of rubrics. Included are not only the cabinet minutes, but also legislative drafts, internal chancellery memoranda, and correspondence with other government agencies, the governments of the federal states, trade unions, lobbies, and other pressure groups. Taken together, these convey a sense of the circum-

stances in which decisions were made that can easily be missed if one is investigating a specific topic, reading only the apposite files in the Bundesarchiv.

Those embarking upon studies of limited topics must still have recourse to the full archival collection, but they, too, will profit from these volumes. By means of the exhaustive indexes of names and topics with which each volume has been provided, it is possible to follow a given theme throughout the tenure of each cabinet and identify those archival files where additional information on it may be found. Also illuminating are the extensive introductory essays by each volume editor. These contain an account of the origins of the cabinet in question, identifying information about each minister, and a description of the major issues that confronted the cabinet, with references to key documents.

Since the cabinet minutes themselves usually consist of laconic, not to say bland, summaries rather than verbatim records of what was said, the decision to supplement them with other documents was a wise one. By reading memoranda and correspondence surrounding a cabinet meeting, one often becomes aware of differences that were minimized or concealed—possibly deliberately—in preparing the minutes. A particularly difficult problem was presented by the cabinet of Philipp Scheidemann, for whose early meetings in 1919 minutes were either not kept or have since been lost. The editor of that volume has done a commendable job of finding substitutes for the missing records by drawing upon a variety of other documentary collections and published memoirs. This is one of the rare instances in which the editorial principle of limiting the volumes to material from the Reich Chancellery collection is—of necessity—extensively violated.

There is little to criticize in this carefully conceived and scrupulously executed undertaking. Two minor cavils can, however, be raised. It is unfortunate that only two of the volumes thus far published (those covering the Cuno and Scheidemann cabinets) contain Erdmann's informative foreword about the past vicissitudes of the documents of the chancellery, about that office's structure and procedures, and about the project's editorial principles. It would enhance the value of future volumes if this foreword

could be reprinted in each. Also useful would be the inclusion in each subsequent volume of basic bibliographical information about all of the preceding volumes, something unaccountably missing in those thus far released. This would be especially helpful to those wishing to order back volumes.

These minor deficiencies in no way detract from this admirable collection's value as a quarry of information for a wide variety of historical inquiries. It will be an indispensable research tool and a boon to the training of advanced research students. One can only hope that it will also inspire other countries to emulate the fine example it sets.

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BARTON WHALEY. *Codeword Barbarossa*. Cambridge, Mass.: MIT Press. 1973. Pp. 376. \$10.00.

Although rather narrow in time and topic, Dr. Whaley's book is a valuable supplement to the broader diplomatic histories. And the authors of future, more general accounts will find it very useful indeed. It is a detailed study of how Hitler achieved strategic surprise in his attack on the Soviet Union.

The author carefully examines the various intelligence services and the information they obtained. Almost everyone, it seems, was reading everyone else's telegrams. Only the important Soviet codes remained unbroken. Dr. Whaley devotes three chapters to the many warnings Stalin received: from the United States, from Churchill, and from his own agents in Switzerland and Japan. Hitler succeeded, the author concludes, not in concealing his troop concentrations but in deliberately fooling Stalin regarding their aim. Stalin apparently believed Hitler would make certain demands, and the build-up was for the purpose of enforcing them. General Antonescu shared this same belief until June 11, 1941; the Finns until about June 15.

An interesting question remains. Why did this explanation make so much sense? As he intimated to Cripps on July 1, 1940, Stalin was convinced that the British blockade prevented Hitler from consolidating his conquests. As long as it continued, Germany and Western Europe depended on the Soviet Union for food and raw

materials. And these could be secured in greater volume, and much more easily, by blackmail than by force.

It still is not generally realized, however, that Hitler based his decision to attack Russia on sound strategic grounds: Molotov made his demands in Berlin in November 1940 during the first Axis reverses of the war (the Greeks were driving the Italians back into Albania). This convinced Hitler that he could not trust the Russians. "They can't call the tune on their own," he declared on December 5, "but they will exploit every opportunity to weaken the position of the Axis." At the time he also expected eventual American intervention in the war. The obvious solution was to conquer Russia before this happened. Then, with assured supplies of food and raw material, and with Germany's rear secure, Hitler could easily defend himself against an Anglo-American alliance in the west.

The moral of this story is simple. When they have a choice, policy makers should not plan on the basis of what they think their opponent will do but on what he can do.

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MICHAEL BALFOUR and JULIAN FRISBY. *Helmuth von Moltke: A Leader against Hitler*. [New York:] St. Martin's Press. 1972. Pp. x, 388. \$16.95.

This is an important addition to the growing literature on the German Resistance movement. It was natural that historians concentrate first on people involved in the abortive putsch of July 20, 1944. Now a very different group is receiving the attention that it deserves. Ger van Roon's *Count von Moltke and the Kreisau Circle* (1971) dealt primarily with the ideas and activities of the group as a whole. The present biography rounds out the picture by tracing the life of Moltke as an individual.

Named after the Moltke family estate in Silesia, the Kreisau Circle was not a tightly organized band of conspirators but an informal association of well-placed young Germans concerned about their country's future. Their aim was not to engineer the overthrow of Hitler (they were not in agreement about the method or even the wisdom of doing that). Instead,

they were engaged in planning the new Germany that they wanted to see replace the Third Reich, however its end might come.

Helmuth von Moltke was the moving spirit of the group, and he typified it in his person: he bore a historic name, was young (only thirty-seven when he was executed in January 1945), held a second-echelon government post (as the expert on international law in Admiral Canaris's *Abwehr*), and believed that Germany could only become capable of stable and civilized self-government after a moral rejuvenation based on Christian principles. He also believed that the country must be politically decentralized, since only in the "face-to-face community" could the citizen achieve true participation and experience. Finally, the new Germany must rest on a fully open society with equity for all (Moltke had actually voted for the communist Thälmann in the presidential election of 1932!).

The great merit of this biography is to show how a Prussian aristocrat arrived at these views, so foreign to the traditional outlook of the ruling class and so different from the motives of many in the military wing of the Resistance. A good deal of the story is told in Moltke's own words through extracts from his voluminous correspondence, especially with his wife. What emerges is not so much an account of events—though Moltke's travels through occupied Europe are fascinating and the description (pp. 216–22) of what life is like in a totalitarian state is the best that I have seen—as of the development and steeling of a noble character.

Throughout the book the authors use the German word *Widerstand* to remind the reader that resistance inside the Third Reich was necessarily different from that in conquered countries. The term is particularly apt when applied to Moltke and the Kreisau Circle. Their "stand-against" the Nazi regime was moral rather than practical. Though Moltke did communicate with the enemy and though he used his office to ameliorate the lot of hostages, POW's, and forced laborers, this is not why he was indicted and executed. The real reason was stated at the trial by the infamous Judge Freisler: "The mask is off. . . . Only in one respect are we and Christianity alike: we demand the whole man." Moltke and his circle possessed humanity and conscience; this was

too much for their survival under the Third Reich. As Moltke said in a moving last letter to his wife, he was to be killed not for what he had done but for what he had thought.

For the authors, this book was a labor of love in the literal sense: both knew Helmuth and the von Moltke family intimately. In the preface they state their desire that the reader should "through the book come to know the man." They have succeeded. Countess von Moltke's appendix, "The Last Months in Kreisau," not only provides a satisfying end to the story but also shows why this remarkable man should have felt the need to communicate almost daily with an equally remarkable woman.

ROBERT E. NEIL
Oberlin College

PAUL P. BERNARD. *Jesuits and Jacobins: Enlightenment and Enlightened Despotism in Austria*. Urbana: University of Illinois Press. 1971. Pp. ix, 198. \$7.50.

Jacobins in Austria, judging by the number of recent works on this subject, still stir imaginations. But Bernard is not really all that concerned with them; his title is misleading since he deals less with Jesuits and Jacobins than with what was in between. Using many contemporary publications, he makes not only a cause but some assumptions—such as that historians have more or less neglected an Austrian Enlightenment intelligentsia, to which he then does justice. Arranged somewhat forcibly into chapter categories are a series of vignettes of Austrian literati worth being written about. Their aims, however, whether utilitarian or humanitarian, are not always clear.

Drawn mainly from the bureaucracy, frequently trained by Jesuits or associated at one time or another with Masons and Illuminati, Austria's social-political critics looked to the state for redress. Despite a capricious censorship that briefly loosened early in Joseph's sole reign, they poured out their occasionally insolent products, mostly, to be sure, the expected attacks on intolerance, inequality, birth privilege, peasant abuse, feudal barbarity, guild restriction, and clerical presumption. They ranted, ridiculed, and rationalized as partisans, as applauders or, even, as outdoers of Josephinian reform. When, cresting in a *Broschurenflut*, the

publication honeymoon ended in the mid-eighties with Joseph's clamping down on Masons and a toughened censorship, skepticism mixed with disenchantment. Bernard's array of mostly petty writers does include the inevitable and imposing Sonnenfels—"for a time one of the great German liberals"—and culminates with a most perceptive social critic—Joseph Richter, the author of the provocative *Why Is Kaiser Joseph Not Loved by His People?* This "complete Josephinian" was also a clever adapter, but more important, a mean between moderates and radicals.

Yet, as Bernard emphasizes, it was not a Jacobin revolution that threatened Austria in 1790 but rather one by the national estates. There were indeed a few Austrian Jacobins and they were not merely imitators of the French; they had native roots. Of course, of the various small groups declaiming on tyranny, not all those using the radical arguments of the Enlightenment were necessarily either radical or enlightened. Bernard insists that the literati had nothing to do with the supposed Jacobin conspiracy and, rather playing down the idea of a disgruntled fourth estate, he terms as nonsense the notion that the absurd conspiracy charade was the logical consequence of the Josephinian Enlightenment.

Reviewing a spectrum of not always accurate historical opinions, Bernard concludes on an eclectic note, that one cannot ignore the Austrian literati who were in some ways more genuine than Joseph and who helped create a climate favorable to reform, and who, one might add, despite all their complaining, were something more than the typical *Wiener Raunzer*. After all, as Bernard indicates, though their political ambitions were as modest as their literary quality, in their own Austrian way they had made their point. And that is the point of this useful, compact book.

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OLIVER LOGAN. *Culture and Society in Venice, 1470-1790: The Renaissance and Its Heritage*. New York: Charles Scribner's Sons. 1972. Pp. 344. \$12.50.

Oliver Logan's study of Venetian culture and society in the last three centuries of the re-

public is a splendid, scholarly, and suggestive work; splendid because it fully samples the feast of that sumptuous civilization, scholarly because it re-evaluates old formulas, drawing on archival and contemporary sources as well as recent studies, and suggestive because a multiplicity of interdisciplinary bridges marks Mr. Logan's map of Venetian life. In his first chapter on the mythology of Venice he sketches the mythic profile of the *città galante*, rich and Epicurean, enjoying perpetual liberty and a perfectly balanced constitution. Briefly summarizing the governmental structure and social framework from which these idealized images of *stato di libertà* and *stato misto* derive, he traces the myth's formulation in Venetian and Florentine accounts of the early sixteenth century. Much of its potency and creativity he attributes to the very uncertainty of the city's nature, "which enabled men to see in her the concrete confirmation of their own different ideals." But it is not that myth which concerns Mr. Logan so much as a scrutiny of the social and cultural realities of the city and a definition of the erudite and austere Venetian ethos. The tensions between the active and contemplative lives that Venetian society experienced and expressed, the scholarly institutions it fostered, and the generations of Barbari, Bembi, and Grimani who maintained its intellectual commitments, occupy the early chapters, along with the cultural role of the Venetian dominions, a crucial matter that provides a leitmotiv of the whole work.

The major emphasis falls upon the relationship of this Venetian and provincial society to literature, the visual arts, and music. A long chapter is devoted to patronage and the collecting of art, and this section is amplified by a substantial appendix listing art patrons of Venice and the Veneto in the sixteenth and early seventeenth centuries. Here Mr. Logan's heroes may be found: "the great antiquarian Federico Contarini," "the great Alvise Corner," "the great Leonardo Donà." Mr. Logan's use of wills and contemporary inventories such as the *Anonimo Morelliano* is exemplary. Not only has he tapped these often neglected resources, but he goes beyond mere cataloging and at every occasion seeks out possible implications of his cultural evidence. Did the antiquarian mania of the later sixteenth cen-

ture reflect the uncertainties of international trade? "Heavy artistic expenditure is a sign of recent wealth, but not necessarily of expanding or stable wealth." Was not art collecting, as represented by the "studio," an acceptable form of expenditure as opposed to ostentatious weddings and extravagant female dress? Such broader considerations are accompanied by close analyses of the artistic, literary, and musical traditions: the Venetian interpretations of *colore* and *disegno*, the development of *popolaresco* comedy, and the qualifications and salaries of *maestri di capella*. The final chapter, following this same method of analysis and hypothesis, returns to the theme of Venetian mythology in its eighteenth-century guise. No longer is Venice the central subject of pictorial apotheoses, but families such as the Pisani and Rezzonica rise to glory on the ceilings of private *palazzi*. The old Venetian aspiration toward grandeur has been abandoned, and the book ends, as do most books on this period of Venetian history, with references to nostalgia and escapism.

One might ask for more specific page references: where is the English ambassador's remark that the passion for luxury increased as trade declined? Where does Sanudo (in his fifty-eight volumes) describe the performances of Ruzzante's comedies in Venice? One might, in conclusion, speculate that Venice in the eighteenth century had less to mourn and escape than we tend to provide, submitting as we do to those poetical and mythical resonances we so ably identify. Yet a myth may have a certain likelihood, and insatiety and speculation are surely profitable fruits of this valuable addition to the field of Venetian studies.

PATRICIA H. LABALME
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DERMOT FENLON, *Heresy and Obedience in Tridentine Italy: Cardinal Pole and the Counter Reformation*. New York: Cambridge University Press, 1972. Pp. xiii, 300. \$19.50.

This monograph is a study of Pole's Italian career, by far the greatest part of his adult life, and, secondarily, it claims to be an investigation of the influence of the Protestant Reformation on Pole and his circle. The book is fundamentally an intellectual biography of Pole, and

of other figures insofar as they touched him. After an analysis of Italian evangelism, which the author defines as an attempt to accept Luther's justification by faith within the framework of the Church, Fenlon moves to Pole. Not trained in theology, Pole came to his religious convictions from a humanist background and from Scripture. For Pole, faith justified and love directed men to good works, while the fundamental question of whether works were meritorious was left unanswered. At the same time, Pole was unswervingly loyal to the papacy and Catholicism and, to a certain extent, did not seem aware of the contradictions in his thought. Held fast by the twin anchors of justification by faith and fidelity to Rome, Pole would sometimes equivocate and dissimulate in order to avoid difficulty. He looked forward to the council for approbation of his own views and for Christian union, but was grievously disappointed. From that point, although he came close to the papal tiara in 1550, Pole was increasingly the target of slander and suspicion from both Italian Protestant exiles and zealots like Cardinal Carafa. Fenlon concludes with a picture of Pole as a sincerely religious if difficult man in whom caution and intransigence stood side by side. Ineffective as a public figure, fate put him into an era and a role for which he was unsuited. Twentieth-century religious developments perhaps will stimulate in contemporary men more sympathy and understanding for Pole than he enjoyed in his own time.

Fenlon accomplishes his major purpose, that is, to provide a religious biography of Pole. He has not uncovered any new sources, nor is the essential story novel, but he has read carefully and discriminately, and his analysis provides new insights. For example, his account of the authorship of the *Beneficio di Cristo* is perceptive and convincing. The major weakness, one which does not detract substantially, is that the author's treatment of the Italian religious milieu is sketchy. Bibliographical deficiencies sometimes lead him to make dubious judgments on the extent and nature of Italian Protestantism. Although Pole and his circle were the most unworldly of men and women, more information on ecclesiastical politics would also have helped.

In summary, this is a competent monograph

that accomplishes most of what it set out to do.

PAUL F. GRENDLER

University of Toronto

HUBERTUS BERGWITZ. *Die Partisanenrepublik Ossola: Vom 10. September bis zum 23. Oktober 1944*. With a foreword by EDGAR ROSEN. (Veröffentlichungen des Institutes für Sozialgeschichte, Braunschweig.) [Hanover:] Verlag für Literatur und Zeitgeschehen. 1972. Pp. 165.

This little book, well-documented, sober, and lucid, is a valuable contribution to the growing literature enabling post-World War II generations to understand the wartime European Resistance. The Alpine valley of Ossola (or Valdossola), wedged on two sides between Swiss territory, was one of several areas in German-occupied northern Italy that was liberated for a brief period by partisans in 1944 and in which an antifascist provisional government successfully ensured the functioning of public administration. As evidenced by hundreds of footnotes and by a six-page bibliography, the author has diligently collected from Italian (both partisan and fascist), German, and Swiss sources the information available on Ossola during the few weeks in September and October between the formal establishment of a five-party coalition junta, which recognized the authority of the Italian government in Rome and cooperated with the Allies, and the German reoccupation of the area. Although the author describes the composite miniscule partisan army (three to four thousand men) and its poor armament and summarizes the major phases of the German-fascist offensive, his main interest is not military but civilian: the structure of the administration and the policies of the junta concerning law and order, the judiciary, the schools, local finances, and commercial exchanges with Switzerland—the only source of supplies (several Swiss citizens helped the junta). The author rightly sees in the “republic” the expression of what—rhetoric apart—the Resistance generally wanted to achieve politically: democracy as defined by Lincoln. The experiences in Ossola and the other “republics” prepared the ground for the democratic republican constitution of 1947. The book is valuable in another aspect: the distinction between Italians and Germans is unimportant; what matters is the conflict be-

tween democracy and dictatorship. It is a pleasure to see postwar scholars transcending the narrow nationalism that until now has prevented Europeans from seeing the influence one nation has had on the others and that has been a major source of distortions.

M. SALVADORI

Smith College

VLAD GEORGESCU. *Ideile politice și iluminismul în principatele române, 1750–1831* [Political Ideas and the Enlightenment in the Romanian Principalities, 1750–1831]. (Academia de Științe Sociale și Politice a Republicii Socialiste România. Institutul de Studii Sud-Est Europene. Biblioteca Istorică 32.) Bucharest: Editura Academiei Republicii Socialiste România. 1972. Pp. 226. Lei 14.50.

VLAD GEORGESCU. *Political Ideas and the Enlightenment in the Romanian Principalities (1750–1831)*. (East European Monographs, 1.) Boulder, Colo.: East European Quarterly; distrib. by Columbia University Press, New York. 1971. Pp. 232. \$7.50.

In recent years the place of the Romanians in the eighteenth-century European Enlightenment has been the subject of many scholarly Romanian works. Generally, the emphasis has been upon intellectual and literary influences. The book here under review explores an aspect—political thought—that has hitherto lacked a synthetic treatment based upon a thorough investigation of contemporary writings. Georgescu has now to some extent filled that gap. He has examined the works of over seventy authors and in the process has rescued a number of them and their works from undeserved obscurity.

The first section of the book sketches the social and political background of the period, and the second offers a composite portrait of the intellectuals and describes the sources of their ideas. Two themes constantly recur: the influence of the Western European Enlightenment on Romanian social and political thought and the nature of the so-called Phanariot regime and its effects upon the intellectual and cultural life of the principalities. The author's treatment of both is incomplete and at times vague. For example, the great problems of the Enlightenment (and of the post-Enlightenment period as well) are described in too cursory a manner to enable the reader to place

the works of Romanian intellectuals in proper perspective. It is also difficult to share the author's condemnation of the Phanariot period as decadent because, as he asserts, it interrupted the "natural course" of development of cultural and intellectual life in the principalities, which was gradually turning toward the West. Georgescu does not make a convincing case. To do so, he must show that indeed such was the trend of Romanian development, a task that requires, among other things, an examination of the importance of Orthodoxy (in the broad meaning of the term) and an analysis of the Greek world of the period.

The best part of the book is the third and final section where the author analyzes the thought of Romanian intellectuals topically and in detail. He stresses the fact that Romanian political thinkers during the period were little given to theorizing but were instead concerned with the practical political problems confronting Moldavia and Wallachia. As a result, their writings deal mainly, though by no means exclusively, with the international situation of the principalities, especially their relationship to the suzerain Ottoman Empire, and with their internal political organization. Of interest also is the short chapter devoted to their ideas about the nature and evolution of human society. A short introductory essay on the historiography of the problems treated and a lengthy, but unannotated, bibliography are useful tools for further research.

KEITH HITCHINS
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VĚRA OLIVOVÁ. *The Doomed Democracy: Czechoslovakia in a Disrupted Europe, 1914-38*. Translated by GEORGE THEINER. Montreal: McGill-Queen's University Press. 1972. Pp. 276 \$12.50.

JIRÍ PELIKÁN, edited and with an introduction by. *The Secret Vysočany Congress: Proceedings and Documents of the Extraordinary Fourteenth Congress of the Communist Party of Czechoslovakia, 22 August 1968*. Translated from the Czech by GEORGE THEINER and DERYCK VINEY. New York: St. Martin's Press. 1972. Pp. vi, 303. \$11.95.

GALIA GOLAN. *The Czechoslovak Reform Movement: Communism in Crisis, 1962-1968*. (Soviet and East European Studies.) New York: Cam-

bridge University Press. 1971. Pp. viii, 349. \$16.50.

Few periods in the recent past have already become so indisputably part of history as the 1968 Czechoslovak "spring." The Soviet intervention in August of that year cut the continuity of development, thus making the preceding events a fascinating, though tantalizing, object of inquiry. In different ways the three books reviewed here suggest three major clues about why this happened—or, in Marxist terms, was "bound" to happen.

Olivová's study is an interpretive survey of Europe's interwar history as seen from the author's Prague vantage point. This perspective is the source of both strength and weakness. On the positive side, what she says about events in Czechoslovakia is often enlightening for Western readers. Her account of them is original and reliable, the regrettable absence of source references notwithstanding. But Olivová is somewhat less than refined in relating Czechoslovak to general European problems—a defect only partly explicable by her insufficient access to Western literature. Indeed, the amount of the malice she imputes to the Right and of the virtue she attributes to the Left makes her writing occasionally reminiscent of history "as little Moritz imagines it." Thus, for example, Hitler's intervention in the Spanish Civil War allegedly served to clear the way for his advance into Africa and Asia (p. 213). And Czechoslovakia's prewar alliances with France and Russia were supposedly formidable because they enjoyed the support of the Communists (p. 172).

Yet too exacting academic standards should not perhaps be applied to a book that is, above all, a passionate confession of faith—that almost mystical Eastern European faith in traditions. The author's two heroes symbolize two traditions that inspired the Czechoslovak Communist reformers. One is Masaryk, the nation's first president and for most Czechs the embodiment of their democracy, the other the Soviet Union, for most Marxists still the epitome of the best in communism. The co-existence of these two strange bedfellows on the pages of Olivová's book is symptomatic of the reformers' self-delusive ambition to reconcile democracy with their devotion to the

"fatherland of socialism"—the ambition that caused them to misjudge so disastrously Moscow's intentions in August 1968.

The volume of documentation about the secret party congress, which met in the wake of the catastrophe in a Prague factory under the noses of the unsuspecting Russian invaders, is somewhat disappointing. Contrary to the assertion on the flip cover, it contains little of "pivotal significance." But it would really be unfair to expect that any such problems could have been discussed and disposed of at a meeting under such extraordinary circumstances. It was enough that the congress met at all and confirmed a reformist majority in the central committee. Otherwise, the fundamental discussions had actually preceded it, and their results were merely summarized in some of the documents prepared for the occasion.

Both these documents and the proceedings give glimpses of a critical problem that faced the party—that of overcoming its disreputable past, for the reform movement had brought to light all too many embarrassing misdeeds, which, to be sure, had been abetted also by quite a few of the reformers themselves in their earlier days. Although the new leadership displayed an unprecedented candor, it still lacked the nerve and perhaps even the ability to face the whole truth and its probable political consequences. On the one hand, for example, a special report to the congress admitted that Czechoslovakia in the late 1950s had been conspicuously less inclined to exploit the opportunities for reform than other Communist nations. On the other hand, however, the same report glossed over the sordid circumstances of the Czech Communists' rise to power in 1948—circumstances likely to account for some of the later deficiencies in their moral fiber.

The third book is by an Israeli author—the citizen of a country whose political and intellectual elite has particularly strong links with Czechoslovakia. A political analyst rather than a historian, Golan used superb documentation including a variety of Czechoslovak press and radio reports monitored in the West. Hers is a sympathetic yet unsentimental account, respectful of the hard facts and free of dubious generalizations. Unlike Vladimir V. Kusin, the

author of the best study on Czechoslovakia's intellectual ferment, Golan focuses upon the equally momentous institutional and organizational changes. She covers in systematic detail the time until January 1968 and adds a sketchy epilogue on the subsequent events, which she proposes to discuss in a separate book. The copious material is well organized in both chronological and topical fashion. More than most authors—who overemphasize the Czech character of the reform movement—Golan gives weight to its less ideological, but equally dynamic, Slovak component.

Golan explains the dramatic developments that eventually culminated in the unique attempt to abolish the authoritarian structure of the Communist party itself and end its monopoly of government. This innovation, utterly alarming from the Soviet point of view, was a third factor that—besides the reformers' ideologically conditioned misreading of Moscow's intent and the debilitating heritage of their own past—forebode their doom. With hindsight, it is difficult to imagine how they could have acted otherwise. The element of predetermination in their actions makes the three books melancholy but nonetheless rewarding reading.

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WOLF ZEEV RABINOWITSCH. *Lithuanian Hasidism*. Foreword by SIMON DUBNOW. New York: Schocken Books. 1971. Pp. xiii, 263. \$7.00.

Hasidism is a Jewish religious movement that began in the 1730s and, like the revivalist trends of Protestant Europe and the Great Awakening in America, strove to make religion warm and emotional and to create "a religion of the heart" with an underlying mysticism but a decline in normative behavior. In so doing it opposed formalism and challenged some of the established institutions. The leader around whom the followers congregated was the illuminate, the charismatic personality who found his way to God.

Hasidism's development started in southern Poland (Podolia and Volhynia), where, among other things, some splinters of the Schismatics (*Raskol*) of the Russian Greek Orthodox Church had sought refuge from persecution.

Some, mostly external, similarities are discernible between these sects and Hasidism. When the latter penetrated into Lithuania and White Russia in the 1760s, it encountered sharp opposition and persecution that was to last for over a generation from the established Jewish communities.

Hasidism subsequently changed. Discarding much of its permissiveness it developed into the mainstay of orthodox Judaism in Eastern Europe. Leadership became hereditary with son following father, and there was division into dynasties. After World War II, offshoots of Hasidic groups that had survived Hitler and Stalin reached Israel and the United States, where they are trying to continue their way of life.

Dr. Rabinowitsch's book depicts the development of the various strands of Hasidism in Lithuania. (It is a translation of his work in Hebrew previously published in Israel.) The author, who discovered some unknown source materials, published a number of studies all of which he has utilized in this book. He tells about the beginnings of Hasidism in Lithuania and White Russia in the second half of the eighteenth century, the persecution of Hasidim between 1772 and 1800, and the growth of their groups over the years, and he includes English translations of some Hebrew documents. The book traces the development of the Hasidic "dynasties," charting genealogical tables of the leaders. The reader can find here a great many details about numerous Hasidic groups and dynasties, some of which stretched over periods of almost two centuries.

Such fragmentation into many small pieces of biographical information, however, greatly detracts from the book's worth. The modern reader is less interested in the biographical notes about over one hundred Hasidic leaders than in a synthetic picture of the movement and its main ideas and philosophy. It would also have been advisable to tell the story in comparative terms, with Western thought trends to facilitate understanding for those who are not familiar with Hebrew texts and esoteric thought.

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WAYNE S. VUCINICH, editor. *Russia and Asia: Essays on the Influence of Russia on the Asian Peoples*. (Hoover Institution Publications 107.) Stanford: Hoover Institution Press, Stanford University. 1972. Pp. xiv, 521. \$15.00.

The ten essays in this volume were read at a conference at Stanford University in 1967 on "The Russian Impact on Asia." The contributors examine the Russian view of Asia (N. V. Riasanovsky); Oriental studies in Russia and the Soviet Union (Richard N. Frye and Wayne S. Vucinich); and the impact of Russia on the European Muslims (Alexandre Bennigsen), Armenia (Vartan Gregorian), Georgia (David M. Lang), Central Asia (Manuel Sarkisyanz), the peoples of Siberia and the Far East (Stephen P. and Ethel Dunn), China (Mark Mancall), and Japan (George A. Lensen).

The list of participants is impressive, and the information imparted is wide-ranging, but the complex subject and manifold approaches make the contributions somewhat uneven. No attempt is made to define the task and the criteria involved. Kliuchevskii's distinction between "contact" and "influence," and the concepts of diffusion familiar to cultural anthropologists could have provided bases for such a standard. As it is, only Stephen and Ethel Dunn discuss methodology.

Without guidelines certain contributors dwell on diplomatic, political, and military events instead of getting down to the means and tempo of transmission, receptivity, and other matters closer to the conference theme. Sarkisyanz, for example, could have assumed more knowledge by his readers of the size and ethnic and cultural diversity of Central Asia and devoted more time to his theme of transformation and acculturation. For this he might have used Barthold's highly relevant *Istoriia kul'turnoi zhizni Turkestana* (Leningrad, 1927). He might also have cited several pertinent works that have appeared during the long period since the conference, by Elizabeth E. Bacon, Violet Conolly, or Teresa Rakowska-Harmstone. Sarkisyanz is the only one, however, who cites Walter Kolarz's keen analyses, based on Soviet sources, in *Russia and her Colonies* (1952) and *Peoples of the Soviet Far East* (1953), which were relevant to several of the essays.

The peoples discussed differ widely as regards Russian influence. Some have been under

Russian rule for centuries (the Kazan Tatars), some never; some are numerically insignificant (the Siberian peoples)—though interesting nonetheless—yet receive space equivalent to that allotted much larger groups, notably the Chinese. Some, like the Armenians and Georgians, are hardly “Asian” at all—there might as well have been an article on the influence of Russia on Israel. The Turks and Persians were discussed at the conference but are omitted from the book, perhaps because of negligible impact. Russian influence on some of the other peoples discussed has also been tenuous.

Thus, Mancall asserts that the Russian impact on China has been “tremendous,” but most of what he relates is political and diplomatic rather than cultural. Attempting to analyze this impact “within the context of particular configurations in the total paradigmatic perceptual scheme through which the Chinese viewed themselves and reality,” he states that Western European culture brought a perception of “diachronic conflict and discontinuity, the past versus the present, the old versus the new,” while the Russian influence introduced a “synchronic” element of class struggle within Chinese society. Chinese culture, however, has resisted outside influence since ancient times, Chinese scholars were conscious of the distinction between old and new, and the country has a long history of struggle between landlord and tenant, of peasant rebellions, and of secret societies. It would seem difficult to attribute the inculcation of such characteristics either to the West or to Russia.

Inevitably there was disagreement on certain points. Bennigsen alleges the Soviet policy of destroying the heritage of the past of minority peoples by changing alphabets, introducing Russian words into written language, and other manifestations of a policy of integration and eventual assimilation. Frye on the other hand dismisses any idea of the Soviet government forcing minorities to become Russianized. Sarkisyanz, although decrying those who see “the division of Russian Central Asia into ethnic territorial units as a mere Machiavellian device to split a ‘Turkestan’ allegedly constituting a natural unity,” notes nevertheless the Stalinist prohibition of economic associations among the several republics. Lang points out the material advance of Georgia under Soviet rule, which

could apply to most of the other republics.

The inherent shortcomings, however, of any conference of this nature and the predictable differences of opinion should not obscure the fact that this is an interesting and valuable book. Aside from their intended stress on Russian influence the essays provide a high-level survey of Russian history and policy toward minorities and neighboring peoples that will be useful for reference and as a source of ideas on a wide range of subjects. Skillful editing has minimized overlapping and differences in translation. There are abundant notes (pp. 369–465) and a satisfactory index.

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L. L. MURAV'eva. *Derevenskaia promyshlennost' tsentral'noi Rossii vtoroi poloviny XVII v.* [Rural Industry in Central Russia in the Second Half of the 17th Century]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo “Nauka.” 1971. Pp. 204.

This book is a detailed and comprehensive survey of the character and geographical distribution of village industry in the central regions of Russia during the second half of the seventeenth century. It is a valuable supplement to the work of Iu. V. Got'e on this topic, for the author exhausts all presently known sources and does not hesitate to list the names of hundreds of villages and settlements and the type of economic activity to be found in each. The author proceeds from a basic assumption of Soviet historiography—that in the second half of the seventeenth century there occurred in Russia an upswing in industrial activity, a more sophisticated division of labor, the creation of a national market for goods, and the beginnings of Russian capitalism. Murav'eva's contributions to this assumption are that much of this economic activity could be found in serf villages and that many peasant settlements resembled commercial towns in the character of their industrial production and the division of labor. The evidence is clear that such economic activity occurred in the villages of the seventeenth century. What is not proven by the author or by Soviet historiography in general is the assumption that there actually was an upswing in industrial production in that cen-

ture. One could, perhaps, more readily accept the arguments of D. P. Makovskii and Got'e, who hold that production at the end of the seventeenth century was no more advanced than in the sixteenth. Murav'eva states that village production in the seventeenth century became more common and typical of central Russia than it was in the sixteenth century, when it was, she says, more "sporadic." In fact, however, what is more typical of the seventeenth century is the availability of source materials, and what is typical of the sixteenth is the "sporadic" survival of records.

THOMAS ESPER

Case Western Reserve University

J. G. PURVES and D. A. WEST, editors. *War and Society in Nineteenth Century Russian Empire: Selected Papers Presented in a Seminar Held at McGill University, 1969-1971*. Toronto: New Review Books. 1972. Pp. 188. \$7.50.

This volume of essays deals with the effect of Russian life and society upon the military system. In the opening article, J. G. Purves analyzes Russia's problems in expanding and modernizing her military establishment throughout the century. He holds that, in spite of costly efforts, it usually lagged because of technical, educational, and economic backwardness and the transportation problem. Hryhorii Fil' indicates that up to 1856, religion was effective in ensuring the loyalty and devotion of the army, but he does not discuss its efficacy against revolutionary influences affecting the army at the end of the century. D. Fattal finds that Bakunin, Kropotkin, and Tkachev believed that armies and wars were by-products of the state, which was the real enemy. This problem seems to require wider treatment.

Dr. West presents an administrative history of the military system under Alexander I during the frantic effort to centralize, modernize, and expand it to prepare for war with Napoleon—a subject that has had little previous attention. In the Ukraine, however, the government decentralized by urging the people to form troops to combat the French. Roman Serbyn believes that the people hoped thereby to recover their lost freedoms. Similarly, E. W. Laine, using Finnish material, shows that Nicholas and especially Alexander II treated the Finns mildly and won their loyalty. It was not until the

German danger appeared that the Russians tried to force military control on the Finns. P. Hidas, using Magyar sources, states that in 1849 the Russians had the sympathy of the Slavs and, thanks to the Russians' discipline and their kindness to the vanquished, of the Magyars. With the Austrians, however, they were often at odds.

Essays by M. McMullen and J. Soley on the role of British opinion and Palmerston in inciting and inflaming Russophobia during the Crimean War, while well done, offer little that is new. In "The Baltic during the Crimean War," J. Knoppers shows that British power could do little there, except to frighten the Russians into massing troops, which otherwise might have turned the tide in the Crimea.

O. Smal pictures the Slavophiles as believing that Russian Orthodoxy would convert the West. When the West backed the Muslim Turks, however, the Slavophiles urged the Balkan Slavs to rise and, when they did not, to put their trust in Russian arms. After the Crimean War, D. A. Miliutin—no Slavophile—modernized the army while urging peace in Europe, although he favored the less risky expansion in central Asia. According to Alexander Pidhainy, however, Miliutin, as Russia got in deeper and deeper, finally insisted that the army had to fight the Turks, although he warned that it was far from ready. How unready it was A. L. Smith makes all too clear, so that it is amazing that the army eventually won a full victory.

P. Spillberg in "The Nation in Arms" indicates that by the end of the century, Russia had no modern military doctrine but regarded the loyalty and bravery of the rural population as more important than firepower and technique. Finally, J. Desmarais gives a detailed and convincing account of the efforts of France to win Russia as an ally.

These essays are based on original research, and some make useful contributions, although it is doubtful whether the scope and nature of the work justify this form of publication. The editing is not always as careful as it should be.

JOHN SHELTON CURTIS
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GEORGE KATKOV *et al.*, editors. *Russlands Aufbruch ins 20. Jahrhundert: Politik—Gesellschaft*

—*Kultur, 1894–1917*. Olten: Walter-Verlag. 1970. Pp. 347. 25.50 fr. S.

GEORGE KATKOV *et al.*, editors. *Russia Enters the Twentieth Century, 1894–1917*. New York: Schocken Books. 1971. Pp. 352. \$12.00.

For many years before 1933, German scholarship was in the forefront of Western study of Russian history and culture, profiting by and based on the long association between the two states. Perverted and debased for propaganda purposes during the twelve years of Nazi rule, German *Ostforschung* since 1945 has once again taken its place as an indispensable component of serious scholarship on Russia and its past. It is fortunate, indeed, that this should be the case, for German students of things Russian work in a tradition whose depth, duration, and richness lend to their work a solidity sometimes lacking in the writings of the scholars of other Western nations, including our own.

An encouraging sign of the growing recognition of these facts is the publication in excellent English translation of this work, a symposium whose preparation was facilitated by a grant from the International Documentation and Information Center (Interdoc), the Hague. The thirteen contributors include seven West German scholars (Oskar Anweiler, who writes on education, Hans Bräker on the Muslim revival in Russia, Helmut Dahm on philosophy, Erwin Oberländer on political parties, Lothar Schulz on constitutional law, Gerhard Simon on church, state, and society, and Karl C. Thalheim on economic development), three British scholars (Violet Conolly on the nationalities question, Michael Futrell, coauthor of the article on foreign policy, and Harry Willets on the agrarian problem), and three Russian emigré scholars (George Katkov, coauthor of the article on foreign policy, Nikolaus Poppe on the economic and cultural development of Siberia, and Elisabeth Stenbock-Fermor on literature). Together these scholars of varied background provide a broad and at times surprisingly detailed survey of Russian society and the Russian government as they moved toward their final rendezvous with war, revolution, and collapse.

In its general organization as well as in many of its individual contributions the volume is traditional and fact- rather than hypothesis-oriented, a characteristic that springs sharply into view if one contrasts it with a recently

published American symposium covering the same period (Theofanis George Stavrou, ed., *Russia Under the Last Tsar* [1969]). Whereas the basic theme of the American volume is the question of whether or not the Bolshevik Revolution was the inevitable response to the failure of the tsarist regime to solve its most pressing economic and political problems (industrialization and democratization), the authors of the German volume tend to avoid speculation, at most mildly suggesting the possibility that the tsarist regime might have kept going indefinitely, given a less calamitous succession of accidental and avoidable mischances. There is a need, however, for both approaches, and students of the period, including university lecturers, will find the conscientious and sober documentation of the German volume valuable.

While space limitations rule out consideration of each paper, mention should be made of several. Of particular note is Oskar Anweiler's treatment of education, a shortened version of the first three sections of his *Geschichte der Schule und Pädagogik in Russland vom Ende des Zarenreiches bis zum Beginn der Stalin-Ära* (1964). Also outstanding is Violet Conolly's study of the nationalities question, a field to which she has previously made noteworthy contributions. Harry Willets's analysis of the agrarian question reduces to relative clarity a highly complex topic. Of the more tradition-oriented papers I found Lothar Schulz's study of constitutional law particularly satisfying, in part because of its forthright assault on the influential but highly questionable view, dating back to Max Weber, that the Russian government after the 1905 Revolution was merely a "sham-constitutional" regime.

Strikingly absent from both the German and the American symposia, regrettably, is any recognition of the importance for this period of Russian achievements in the visual arts, music, ballet, and the theater—a glaring revelation of the predominantly word- and book-oriented viewpoint of many academic specialists. In both symposia the name of Sergei Diaghilev, the great impresario, makes only a single appearance, as sponsor of the literary-artistic journal, *Mir Iskusstva*. The great names of early twentieth-century Russian music—Rachmaninoff, Prokofiev, Scriabin, Stravinsky, and others—are completely lacking, as are those of such

outstanding painters as Kandinski, Vrubel, Repin, V. A. Serov, Roerich, Bakst, and Benois. Yet one of the basic problems posed by the Russian Revolution has not even been formulated, let alone solved, if one fails to realize that it took place at a time when Russian society, through its creative intelligentsia, was giving proof of unparalleled vitality.

ROBERT M. SLUSSER

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IVAN S. LUBACHKO. *Belorussia under Soviet Rule, 1917-1957*. [Lexington:] University Press of Kentucky. 1972. Pp. 219. \$10.00.

The paucity of English-language works on Belorussia should make this volume welcome. In thirteen brief chapters and a conclusion the author discusses Belorussia's pre-Soviet past and the growth of the national movement, the proclamation of national independence, the formation of the Belorussian Soviet Republic (after two Soviet governments had failed), the partition of the country between Poland and the Soviet Union from 1920 to 1939, and the course of developments in Soviet Belorussia during the 1920s—a period of relative freedom—as well as Stalin's repressive policies in the thirties and forties. The fate of Western Belorussia under Polish rule is discussed along with the reunion of Eastern and Western Belorussia in 1939. Separate chapters are devoted to the cruel German occupation of 1941-44 and to the wartime diplomacy that led to Belorussia's inclusion as a charter member of the United Nations.

Although Lubachko frequently cites the late Nicholas Vakar's *Belorussia: The Making of a Nation* (1956) he has also utilized a wide variety of Soviet Belorussian and Russian sources, emigré publications as well as some interview data. Yet there are certain lacunae. There is little biographical information on individual leaders, whether Communist or nationalist, and they remain names rather than personalities. There is little discussion of the origins of the abortive Soviet effort in 1919 to merge Belorussia and Lithuania in the so-called Litbel. The 1945-57 period is given relatively little attention. Considerable emphasis is given to general aspects of Soviet policy such as the origins of Stalin's nationality policy, collectivization, industrialization, and the purges, but it is pre-

sented in the context of the Belorussian specific. Unfortunately, there remains a lack of Western studies on Soviet Belorussia comparable to the detailed studies of the Ukrainian Soviet Republic by Yaroslav Bilinsky and Hryhory Kostiuk.

Within the limits set for himself, however, Lubachko has written a very useful work. One of its attributes is that it is written from a Belorussian point of view, which simply means that the author documents the costs exacted by Moscow in political and cultural terms. Lubachko's work merits a place among the growing number of specialized studies dealing with the Soviet nationality problem.

JOHN S. RESHETAR, JR.

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NEAR EAST

The Near East in Modern Times. Volume 1, *The Ottoman Empire and the Balkan States to 1900*, by GEORGE G. ARNAKIS; volume 2, *Forty Crucial Years, 1900-1940*, by GEORGE G. ARNAKIS and WAYNE S. VUCINICH. Austin: Pemberton Press. 1969; 1972. Pp. xv, 452; 356. \$12.50 each.

What makes these volumes different from other textbooks and general surveys on the Balkans or the Middle East is the authors' belief that these two areas should not be considered autonomous fields of study but rather should be conceived as a unity. In his preface to volume 1 Professor Arnakis tells us that one of the fundamental premises upon which he bases his study is the concept of a unified approach to what, in traditional terminology, has been referred to as "the Near East." The Near East he defines as the land formed by the converging extremities of the three continents of the Old World—Europe, Asia, and Africa—an area that can be said to fall into six divisions geographically: the Balkan Peninsula, Asia Minor, the Fertile Crescent, the Nile-land, the Arabian Peninsula, and the Iranian Plateau. While Arnakis's geography may not be acceptable to everyone his decision to move away from specialization and survey the larger area seems not only easily defensible but also clearly a move in the right direction. For despite their complexity and diversity the Middle East and the Balkans have often, in the past, formed a historical unit. This was certainly the case to a

greater or lesser degree with the Persian, Alexandrine, Roman, and Byzantine Empires in ancient and medieval times.

But, most recently, it was the Ottoman Turks who held dominion over the whole area for centuries. It is, therefore, on the Ottoman Empire that Arnakis focuses after a brief survey of the Near Eastern peoples, their history, and their conflicting religious heritages. (In this latter connection he sees the contest between Byzantines and Arabs as the "greatest and most protracted culture conflict in recorded time" [p. 47].) Nevertheless, in telling the story of the origins, expansion, institutions, and decline of the Ottomans from the fourteenth to the late eighteenth centuries Arnakis does not seem to have made much use of the latest research of both Western and Turkish historians of the Ottoman Empire. Thus, the "Lybyer thesis," now generally considered outdated and an oversimplification of the facts, is still relied upon to explain the dynamics of the Ottoman ruling elite (p. 68). Similarly the judgment that Bajazet II (1481-1512) was "unequal to the task of keeping the Ottoman Empire together" (p. 63) does not find acceptance today. As for the causes of decline it is no longer sufficient to attribute them primarily to excesses of militarism, favoritism, and corruption without reference to long-range causes such as population growth, inflation, and geographic and logistical limitations to the deployment of Ottoman military power.

Still, most of the first volume—three-fourths of its contents—deals with the emerging nationalisms in the Balkans where the Ottoman Empire first began to disintegrate. And this theme, the basic one of the study, is treated thoroughly. Arnakis provides us with competent accounts of the development of the Greek, Romanian, South Slavic, and Bulgarian national movements in the late eighteenth and early nineteenth centuries that are skillfully interwoven with the complex strands of Ottoman reform and great power diplomacy on the Eastern Question. On the other hand, with the exception of a chapter on Mohammed Ali of Egypt and the European powers, only passing reference is made to conditions in the Arab provinces of the Empire during this time.

In this respect volume 2 is much more soundly balanced. After a detailed examination of the

cataclysmic events leading to the dissolution of the Ottoman Empire and the birth of the Republic of Turkey, developments in Iran, Syria, Lebanon, Iraq, Transjordan, the Arabian Peninsula, Egypt, the Sudan, and Palestine are given as careful attention as those in the Balkans. (The chapters on the Balkan states, with the exception of Greece, are contributed by Professor Vucinich.) There is also a satisfying balance between internal political events, international rivalries, and economic and social conditions. Convenient summaries of cultural, artistic, and intellectual accomplishments in each country are also included.

What emerges from a study of the 1900-40 period as a whole is that the ideology of nationalism, while decisive in bringing about the downfall of the Ottoman Empire, failed to provide answers to the tough economic and social problems faced by the newly independent or mandated states of the Near East—problems aggravated by the Great Depression. In fact, as Arnakis points out, it became fashionable for economists to deplore the collapse of the huge multinational Ottoman and Habsburg Empires that were, in their eyes, much more viable geopolitical entities. Middle-class leadership proved largely ineffective in the Balkans, except for brief periods of beneficial rule in Greece and Romania in the second and third decades of this century. In the Middle East its energies were absorbed by the struggle to shake off British and French control. In both areas dictatorships became the general rule. By the late 1930s this was the case "all the way from the Danube to the Nile and from the Adriatic Sea to the Indian Ocean" (p. 295). The most successful of all these Near Eastern dictators, according to Arnakis, was Mustafa Kemal of Turkey who labored systematically and effectively for the creation of a Westernized, progressive bourgeoisie in his country. Ataturk's accomplishments constitute "the greatest bloodless revolution in the annals of Islam" (p. 295).

Like most general surveys these volumes are not models of readability, although the style is clear enough. And, like other such efforts, it has its errors. Some are seriously misleading—for example, the Persians and the majority of the inhabitants of Iraq did not choose the Shi'a form of Islam after the assassination of Ali (vol. 1, p. 9), but centuries later—while others

are mere slips: the Kurds did not rebel against the Turkish authorities in the summer of 1929 but in 1930 (vol. 2, p. 81).

The usefulness of both volumes is enhanced by such aids as a glossary of terms, a chronology, dynastic tables, and a select bibliography of works available in English. A third volume, *Second World War and after 1940-1960*, has not yet appeared.

DENNIS N. SKIOTIS
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M. K. ZULALIAI. *Armeniiia v pervoi polovine XVI v.* [Armenia in the First Half of the 16th Century]. (Akademiia Nauk SSSR, Institut Vostokovedeniia; Akademiia Nauk Armianskoi SSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 124.

This is a brief analysis of western Armenia, or the eastern provinces of the Ottoman Empire, in the first half of the sixteenth century. The author first describes the social and economic structure of these provinces and then discusses the effects of the Ottoman-Safavid conflicts on the Armenian regions. In the first and more important of the sections the author bases his account mainly on Armenian and other contemporary chronicles, although he uses those published primary Ottoman sources available to Soviet scholars. *Kanun-names*, edited by Omer Barkan, chronicles, and the travelogues of Evliya Çelebi add greatly to the author's evidence. Nevertheless the picture given of the different classes and their interrelationships, especially between Muslim and Armenian, is a contradictory one. Armenian chronicles speak of Muslim tyranny; foreign travelers' accounts and that of Evliya Çelebi point out that the lucrative East-West trade, which had not yet diminished by 1550, was in the hands of "wealthy Armenian merchants," while the Ottoman legal documents fill in the intentions, if not the practice, of the ruling classes as manifested in tax laws, privileges, and the political structure. But what was social and economic life really like in these provinces? Which evidence should we accept? The author does not provide the answers, and with good reason, because until the Ottoman archives themselves are studied, along with records of local officials, the judiciary, and religious establishments, our knowledge of this or any other Ottoman province

will remain incomplete. However, this book is the best treatment to date of these Ottoman Armenian provinces in the sixteenth century.

ALAN W. FISHER
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CARL MAX KORTEPETER. *Ottoman Imperialism during the Reformation: Europe and the Caucasus.* (New York University Studies in Near Eastern Civilization, number 5.) New York: New York University Press. 1972. Pp. xix, 278. \$14.50.

Organized on the scale of a Mongol campaign, this book has as its subject the history of the northeastern and eastern Ottoman frontiers from the end of Suleiman the Lawgiver's reign (1566) to the death of the Crimean khan, Ġāzī Girāy (1608).

From his study of political affairs in the long frontier zone running from Hungary through the Crimean steppe to Azerbaijan, Kortepeter extracted a number of themes by which he seeks to explain the history of Ottoman imperialism. Three of these arguments are important. First, the establishment of Ottoman influence in most of Hungary owed its success, in part, to the Hungarian reaction to Habsburg and papal attempts to impose their control over local populations. Second, the semi-independent status of the Crimean khanate masked an imbalance of power on the steppe frontier resulting from the technological inferiority of Tatar arms. Finally, the urge toward the Black Sea of the Muscovites combined with the rise of Safavid power in Persia to threaten Ottoman connections with Central Asia.

Kortepeter, however, offers meager evidence on how these trends affected Ottoman policy. In his history of the Danubian frontier, for example, the campaigns of the Tatars receive more attention than does the study of why contrasting imperial policies brought most of Hungary under Ottoman rule. Granted that the Tatars did not adopt firearms with alacrity, still the record of their military effectiveness through the early eighteenth century makes one wonder whether or not technological inferiority was a major factor in the steppe politics of the late sixteenth century. More intriguing, however, is the unanswered question concerning the status of the khanate. Why did the Ottomans refrain from imposing direct con-

trol over the Tatars? Equally exciting is the geostrategic issue raised by the Russo-Safavid convergence on the Caucasus. But if this event represented a real danger to the Empire, then should not the 1577 decision to war against the Safavids be judged from a viewpoint other than that represented by court factions opposed to the campaign?

The undue concentration on Crimean affairs in this work draws attention away from the central event underlying imperial history in the age of Ġāzī Girāy (1588–1608)—the cessation of Ottoman expansion. Surely, the ending of approximately three centuries of Ottoman growth is connected with the reasons why the Ottomans were not able to impose direct administrative control over the turbulent border regions of Hungary, the Crimea, and the Caucasus.

ANDREW C. HESS
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RICHARD W. BULLIET. *The Patricians of Nishapur: A Study in Medieval Islamic Social History*. (Harvard Middle Eastern Studies, 16.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xvi, 288. \$12.50.

This short, stimulating, and highly intelligent analysis of social life in Nishapur from the tenth to the twelfth centuries A.D. is an outstanding contribution to Iranian urban history. The first eighty pages define the composition, social motivation, education, and political influence of the city's ruling elite while the remainder of the book is devoted to a meticulously documented reconstruction of the leading families within that elite, their genealogies, marriages, occupations, and sources of income.

At the heart of Bulliet's study—and it is this that gives it so much of its importance—is his rigorous and, to my mind, wholly convincing definition of the ruling elite of Nishapur as a patriciate (pp. 20–27). This patriciate was recruited from three separate groups, with the *ulama* occupying pride of place but deriving additional economic support and political leverage from marriage alliances with the families of the wealthier merchants and of local landowners belonging to the former *dihqan* class. The *ulama* never constituted a closely knit, coherent group—there were subtle distinctions that it is now difficult to perceive clearly—but Bulliet

makes a persuasive case for regarding the right to teach (as opposed to the right to learn) as a measuring rod of social pre-eminence. "The point of control in the system was the determination of who was to get to teach. The certifying apparatus . . . was the very heart of Islamic education of that time" (p. 54). Since successive rulers were willing to allow such cities as Nishapur a considerable degree of autonomy the patriciate had little difficulty in maintaining its grip over local affairs with minimal interference from without or protest from within. Much of this is confirmed by what we know of other cities in the area—Heart, in particular—but it is not the least of Bulliet's virtues that he insists that what he is saying relates specifically to Nishapur and not to the cities of Khurasan as a whole, while nowhere does he offer his readers a "model" of the traditional Islamic city.

Like most scholars who have investigated the social structure of the Iranian city in the early medieval (i.e., pre-Mongol) period Bulliet is clearly baffled by the role of the *madhhab* in the furious faction-fighting that ultimately destroyed the city in 1162; it is indeed difficult to know what to make of these violent and seemingly irrelevant confrontations. Probably similar studies of other major centers such as Tus, Ray, or Isfahan will assist in elucidating the problem as well as confirming *The Patricians of Nishapur* as a scholarly contribution of truly seminal influence.

GAVIN R. G. HAMBLY
Yale University

AFRICA

JAMIL M. ABUN-NASR. *A History of the Maghrib*. New York: Cambridge University Press. 1971. Pp. x, 416. \$14.50.

This general history is an impressive survey of the Maghrib between the time of the Carthaginian commercial empire and the independence of Libya, Morocco, Tunisia, and Algeria in the mid-twentieth century. Directed toward a university audience and based upon the secondary literature in French, English, and Arabic, with an occasional dipping into Arabic sources to verify fact and interpretation, the book is in several respects unique.

Professor Abun-Nasr possesses special insight

into Islam, as well as what it means to be Arab or Berber. His view of motive and action is therefore subtly different from the general works of Western scholars such as Julien, Barbour, Hahn, or Le Tourneau, and adds a new dimension. He also valiantly incorporates Libya into the history of the Maghrib without losing sight of the long-standing connection with Spain. From the Carthaginian through the Ottoman periods the focus is basically political. Abun-Nasr, however, makes an effort to mention major economic and cultural questions within the religio-political framework, but a four-hundred-page survey inevitably imposes limiting decisions. As a result it is reasonably certain that his audience of university students will in some cases lack the staying power to plow through the ever-changing fortunes of multifarious tribal leaders who come and go with the wind. This kind of experience can be of value, as any serious reader of Merovingian history knows, if the end result is intelligent generalization based upon analysis. Usually this quality is present.

For the modern historian the book's pace accelerates two-thirds along the way. By the nineteenth century the sources are much deeper. Further, the monographic work is far richer, and in this context the author skillfully uses the recent work of that small but strong group of American scholars—Ashford, Brown, Gordon, Halstead, Ling, Moore, Charles Stewart, not to omit Graham Stewart. This output, plus the special and general studies of the French and British researchers, gives rise to the thought that along with European occupation of the Maghrib—with its dynamism, ethnocentrism, and destruction of tradition—came an impressive amount of work that today furthers our understanding of the area. This book would suffer without it.

One refreshing characteristic deserving mention is that Abun-Nasr in his final two chapters, which are done in a historian's shorthand, draws his own original conclusions. Thus he considers indigenous nationalism well launched before World War II, and, as a result, he is unwilling to credit the expansion of that conflict to North Africa with any important contribution to independence. I find this view not entirely persuasive. Maghribian nationalism surely drove forward before 1939, but it is

true that the wartime preoccupations of the European (and Asian) powers catalyzed African nationalism. The emerging superpowers, the United States and the Soviet Union, stood on record against colonialism and for national self-determination. This body of opinion no doubt contained much cant, but it aided Maghribian nationalism to the extent that the idea became familiar. The defeat of France in 1940, also, cannot have been without importance, as was the defeat of Italy, for those people reaching toward independence. If Professor Abun-Nasr will look as deeply into recent European history as he has into the history of North Africa, possibly he might temper his judgment. His point is nevertheless well taken. It is consistent with the general message one might draw from the book: Maghribian history is long and complex. Determinists, therefore, should exercise great restraint in pushing their dogma.

RICHARD M. BRACE
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CHARLOTTE A. QUINN. *Mandingo Kingdoms of the Senegambia: Traditionalism, Islam, and European Expansion*. Evanston: Northwestern University Press. 1972. Pp. xxiii, 211. \$10.00.

Based upon a thorough illumination of relevant British and French archives and upon the judicious use of oral data collected in the Gambia this study provides a first highly readable and substantial synthesis of the structure and evolution of Gambian society in the nineteenth century. A description of the pluralistic society of Senegambia precedes histories of the north and south bank states that carefully document their internal structure and the tensions that evolved until open conflict occurred in the 1860s. A chapter on European settlements traces the development of trade interlocking the European and African communities in Bathurst while pointing out that neither the Colonial Office nor local officials understood the realities of the Gambian situation.

Political authority in the Gambia, if greatly decentralized, was the exclusive preserve of the Soninke aristocracy. Civil and military authority were divided; *jawara* generals were elected only for the duration of specific conflicts. As the author shows, frustrations were

keenly felt by a new class of people who in the nineteenth century had achieved wealth outside the traditional clan structure through trade and cultivation and export of groundnuts. Members of this new class were often from the Marabout communities, where people had settled because of occupational and religious interests rather than because of clan ties. Represented in central councils at a lower rank than were men of comparable Soninke title, the Marabout leaders formed an "Islamic shadow state" whose economic power far outweighed its access to political authority.

The jihad inspired and led by Maba Diakhou Islamized the population of the Senegambia and shattered the power of the Mandingo aristocracy. In the second half of the book the author emphasizes the "social revolutionary aspects of the jihad" and studies in further detail the "sharp conflicts which emerged as clan strata began to grow into broader social classes." The "secularization" of the jihad and intense European rivalries—commercial at mid-century but by the 1890s exploding into political and military competition between British and French that resulted in the imposition of colonial rule—were important factors in preventing the existence of an enduring Islamic state.

The author delineates similarities between the Gambian jihad and other nineteenth-century Islamic reform movements, and the discussion of the "secularization" of the jihad raises some interesting questions about differences in the dynamics of post-jihad periods. Dr. Quinn explains the "secularization" by a variety of reasons: the early loss of Maba's religious example; ambivalence toward leadership in Sufi thought and in Maba's own reticence to occupy himself with statebuilding; the traditional Marabout distrust of secular authority; the Mandingo resistance to Wolof expansion of which Maba seemed an example because of his alliance with Lat Dior and other northern contacts; and the European opposition that precluded efforts to consolidate military gains. In Hausaland the potential threat of a Hausa backlash against Fulani expansion did not occur nor did the early withdrawal from political life of Shehu Uthman dan Fodio lead to the collapse of the Sokto Empire. He and his chief lieutenants, Mohammed Bello and

Abdullahi, had long occupied themselves with the proper form an Islamic government should take. Maba did not and his successors less so on the basis of present evidence. Further investigation of Maba's ideas and the Arabic literature and correspondence of the jihad might clarify why the only man who seriously attempted to unify the African powers of the Senegambia seemed to withdraw from these problems. The hierarchical and centralized political and administrative institutions of pre-jihad Hausaland, which enabled the Fulani to assume control without utterly destroying the political fabric of society, were absent in the Gambia. Despite the "Islamic shadow state" and their real economic preponderance (which the Fulani in Nigeria did not have on their side) the Marabouts were unable to overcome the centrifugal tendencies of Gambian political institutions. The new entrepreneurial class, whose emergence during the first half of the century was an important theme in Gambian developments, disappears in the post-jihad era. A future examination of the relationship between it and the Soninke and Marabout leaders during the last thirty years of the century will be useful.

These themes of religious reform, secularization, and the emergence of social classes are only three examples among others which illustrate that this book will be welcomed both for its sensitive groundbreaking study of the nineteenth-century Senegambia and for its contributions to comparative historical work on economic and social as well as political change.

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T. N. TAMUNO. *The Evolution of the Nigerian State: The Southern Phase, 1898-1914*. (Ibadan History Series.) [New York:] Humanities Press. 1972. Pp. xvi, 422. \$12.50.

KENNETH W. J. POST and GEORGE D. JENKINS. *The Price of Liberty: Personality and Politics in Colonial Nigeria*. (African Series Studies, number 7.) New York: Cambridge University Press. 1973. Pp. xii, 494. \$27.50.

These two volumes are curiously complementary. Each deals with a discrete period of Nigerian history, each is concerned with aspects of the creation of the modern Nigerian state,

and, in their different ways, each recounts in detail events that led to crucial stages in Nigerian political development.

Mr. Tamuno takes a fresh look at the rise of the Nigerian state between 1898 and 1914. The book is an administrative history covering in detail the relationship between the colonial administration and the indigenous authorities as British rule was firmly established in southern Nigeria. The author's careful research into the records of the period serves to illumine many aspects of colonial policy, and, in particular, the events leading up to amalgamation.

The main outlines of the history of this period are, of course, well known, but the unique quality of Mr. Tamuno's work is that the narrative of events during the period is seen not from the vantage point of a British defender or attacker of imperial policy, but through African eyes. The role of the leading African political figures of the time is delineated more clearly than in previous histories, and even the prominent personalities of the colonial administrators emerge in somewhat different perspective. The author's comment that "between 1909 and 1913 the British authorities lost a valuable opportunity of making, and gaining from, detailed investigations into the institutions of the people whom they hardly understood and who scarcely understood them" points up a fundamental misconception of the years of indirect rule that followed World War I. Many administrative officers who felt certain that they understood "their Africans" in fact comprehended and appreciated little of the culture of the people over whom they held authority. Mr. Tamuno's work adds a new dimension to historical interpretation of the period he describes, and it is to be hoped that he will see fit to extend his research to the period between the wars.

The Post and Jenkins volume treats much more contemporary Nigerian history. It is a biography of one of the more fascinating figures among the nationalists of the preindependence years, Adegoke Adelabu. The story of Adelabu's rise to political influence and power in Western Nigeria is not only of interest because of the personality of the man, but because the perplexities and frustrations he suffered were, in a real sense, those of many in his time and, more particularly, those of his contemporaries

who entered the political arena at the point of violent eruption of party contention in the Western Region. I can attest from personal experience to the fact that Adelabu was indeed the complex figure portrayed by the authors. He sought power and wealth, and he did not hesitate to use his charisma for his own ends. Yet, at the same time, he was one of the relatively few politicians of the period who retained, in the face of the multiple countercurrents of shifting political loyalties, a vision of the destiny of a united Nigeria. His sudden and tragic death in a motor accident on the treacherous Lagos-Ibadan road robbed Western Nigeria of a figure whose hold on the masses of Ibadan might well have been extended, had the time been given him, to Nigeria as a whole.

This book is not one for the beginner in Nigerian politics; the references are often obscure and unexplained. The detailed account of events is often unnecessary to the main theme and of interest primarily to those who knew Nigeria at this period. The labor of research that went into the writing is obviously prodigious, and, indeed, as the authors imply in their preface, the book is to some extent a labor of love. But these caveats notwithstanding, to trace Adelabu's career is to trace the political history not only of a man but of a community during the feverish years of party activity. Adelabu was not always an attractive figure. His idealism was offset by a ruthless pursuit of ego satisfaction; the successful politician did not signal the emergence of the statesman. But in this he differed little from those with whom he did battle. What set him apart was his energy, his ambition, and his political skills combined with a strain of idealism. The authors have succeeded in creating a vivid portrait of him in the context of the brief time he occupied the political stage. For this they are to be congratulated.

L. GRAY COWAN

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A. H. M. KIRK-GREENE. *Crisis and Conflict in Nigeria: A Documentary Sourcebook, 1966-1969*. Volume 1, January 1966-July 1967; volume 2, July 1967-January 1970. New York: Oxford University Press. 1971. Pp. xiv, 459; vi, 528. \$13.75; \$17.00.

Of only a few works can one genuinely say that they are truly indispensable for the study of their subject, that they are the place to begin. This is one, and it is a remarkable achievement on many grounds. It shows not only an extraordinary amount of careful work, but work under difficult conditions. Kirk-Greene had to deal detachedly with crisis and civil war in a country where people and events mattered to him a lot, for he had spent some twenty years as an administrator and academic in northern Nigeria. It was not work that could be put off to some other time, partly because much material could simply disappear—some into the airwaves, some into the decay that humidity brings, some as casualties of the war itself.

The primary part of the work is more than a collection of 227 documents of the usual type. These are not simply standard items. There are speeches where different versions of texts had to be sorted out, transcripts of elusive radio broadcasts, and statements collected from many locales on three continents and culled sometimes from nearly inaccessible newspaper accounts. There are even facsimiles of ephemeral leaflets and posters that circulated only briefly but could in crisis matter more than any formal speech.

Not that the crucial official documents are omitted; they are all there, and some, most strikingly the Special Branch report on the army coup of January 15, 1966, are particularly important and available nowhere else, even to the avid researcher. Indeed, Kirk-Greene's material on that coup, including Major Nzeogwu's radio broadcast and television interview right after it, are especially valuable, since evidence for the major's views and plans is sparse and likely to remain so.

These volumes make a further contribution. Explicitly designed as a "history towards a history," a guide to fuller research as well as a source book for that research, they present the first full narrative account of that history in long introductions of over one hundred pages per volume. In these essays the author makes no attempt to conceal his point of view. His years in the north particularly affect his presentation of events during 1966 and 1967, especially his assessments of the military coups and their aftermaths. His approach to the

conflict itself can be described, if too simply, as profederal. Two points are salient for him: he believed that the unity of Nigeria should be preserved; he did not believe that the federal government practiced—or intended—genocide.

Fortunately the overelaborate use of metaphor and alliteration with which his presentation begins gives way in the second volume to an unencumbered narrative, more evenhanded in pointing out in footnotes the inconsistencies and ambiguities in both Nigerian and Biafran government positions. These introductions serve also as a way for the reader actually to use the documents themselves, for the volumes are, alas, without an index. More irritating is the absence of a list of documents in volume 1; the reader must apparently have both volumes in hand at all times. The end of volume 2 also contains the extensive annotated bibliography, so important for further researches.

Kirk-Greene's subject has now begun to fall into some perspective. The charges and countercharges have receded. There was no genocide, and the postwar reconstruction and reconciliation have been profoundly impressive, even for those whose faith in Nigeria and Nigerians (not excluding Ibo) was greatest. Historians must resist the temptation to read backward here as elsewhere, of course, and for that reason, among others, the contemporaneity of Kirk-Greene's work adds to its value.

We have not had the expected spate of postwar accounts, but the most important of them (John de St. Jorre, *The Brothers' War* [1972]) acknowledges a debt to Kirk-Greene that others will share. One of the first analyses based on experience inside the Biafran enclave (Arthus Nwankwo, *Nigeria: The Challenge of Biafra* [1972]) offers illuminating insights, the more so when the reader complements them with the documentary context of Kirk-Greene's volumes.

Though most Nigerians—Ibo and non-Ibo alike—have put the war firmly behind them, many of the challenges remain. An African leader talked in 1969 of "the immense significance for the rest of Africa of the Nigerian experiment. . . . Had Nigeria succeeded (and Nigeria still can succeed . . .) . . . we would be able to say: 'Within Nigeria there are several peoples, each conscious of itself and

conscious of its ability to be a nation on its own. If they have nevertheless succeeded in submerging their natural unity into a larger artificial unity, for the greater benefit of them all, then the rest of Africa can submerge its smaller artificial units into that greater artificiality (indeed that more natural unit of all Africa) which holds greater promise for all the peoples of Africa" (vol. 2, p. 436).

It is paradoxical that "Nigeria's real significance to Africa" should have been so clearly stated by President Nyerere of Tanzania, in explaining his country's decision to recognize Biafra. Kirk-Greene's work will help many make sense out of such paradoxes.

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ASIA AND THE EAST

RASHĪD AL-DĪN. *The Successors of Genghis Khan*. Translated from the Persian by JOHN ANDREW BOYLE. (Persian Heritage Series.) New York: Columbia University Press. 1971. Pp. xi, 372. \$12.50.

This is a primary source for Mongol history, long known to Orientalists and Central Asian specialists and now available in English to world historians. "Our chief authority on the origins of the Mongols and the rise of the Mongol World Empire" (p. 8), this work constitutes the second volume of the *Ta'rikh-i Ghāzānī*, according to the "convenient division into three separate volumes, first proposed by E. G. Browne in 1908, . . . adopted by the Russians in their recent editions and translations of the Persian text" (p. 9). The first volume deals with the different Turkish and Mongol tribes (pt. 1) and Genghis Khan and his ancestors (pt. 2), while the third volume covers the Il-khans of Persia, down to the Il-khan Ghazan.

This *Ta'rikh-i Ghāzānī* is itself volume 1 of Rashīd al-Dīn's magnum opus, the *Jāmi' al-Tawarikh*, volume 2 of which, commissioned by Ghazan's successor, Öljeitü, is the first of the universal histories, dealing with all Eurasian peoples with whom the Mongols had come in contact—the Persians, Arabs, Turks, Jews, Franks, and Indians.

This work on Genghis Khan and his suc-

cessors is therefore but a small part of Rashīd al-Dīn's total contribution to Eurasian history, howbeit very important. Covering all those successors from the Great Khan Ögedei (1229–41), third son of Genghis Khan, down to Qubilai's grandson, Temür Öljeitü (1294–1307), Rashīd al-Dīn discusses each ruler under three categories: his wives, sons, and descendants; the details of his life and reign; and anecdotes illustrating his character, sayings, and miscellaneous relevant information.

Professor Boyle, the translator, with the passing of Vladimir Minorsky, is in the forefront of English-speaking authorities on Mongol history, but particularly on its major Persian sources. High standards of editing and translation would be expected from his publication, in two volumes, of Juvaini's *History of the World-Conqueror* (1958). Here his documentary apparatus and interpretive footnotes throughout the translation are evidence of his thorough research and illumination of the historiography of the Mongol period. All historians are indebted to him for this painstaking and perceptive rendition of one of the most important contemporary sources for Mongol and Central Asian history.

It remains to note that the volume contains useful appendixes: a good selective bibliography, four tables of Mongol dynasts, with dates and family relationships, and a fine detailed index.

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HUNG-MAO TIEN. *Government and Politics in Kuomintang China, 1927–1937*. Stanford: Stanford University Press. 1972. Pp. xii, 226. \$8.95.

Professor Tien's study examines the effectiveness of the Nationalist (Kuomintang) party-government in unifying and ruling China during the decade 1927–37. It is not so much a political history of this period as an analysis of institutional development directed to answering the question of the extent to which Kuomintang (KMT) party and government institutions penetrated Chinese society. He concludes that the factional politics characteristic of the Kuomintang, which permitted the personal dominance of Chiang Kai-shek, were an obstacle to gaining genuine political power be-

cause efficiency and administrative ability were often incompatible with loyalty to the party-government leader. Chiang firmly controlled the Kuomintang, but the KMT had only a narrow power base and was compelled to compromise with regional (militarist) interests and conservative social groups. The central administrative bureaucracy was weak and unable to subordinate the provincial administrative machinery to the goal of national development. Such partial control as the center achieved over the provinces was obtained largely by military means. It was, as a consequence, a fragile and superficial unity not founded on the elaboration of viable political institutions. The first decade of Kuomintang rule saw little real nation-building and an almost complete neglect of social reform. By the time war with Japan came in 1937, the eventual disastrous fate of the Kuomintang was already sealed.

To make his case the author describes in turn the failure to develop a new administrative bureaucracy at the center, the hollowness of the formal KMT party organization, the increasing dominance of military interests, competing political factions in the KMT party-government, the pattern of revenues and expenditures of both the central and provincial governments, and the general inability of the civilian party and government to penetrate and control the provincial administration in contrast to the very large role played by Chiang's military power.

I have no quarrel with Professor Tien's conclusions; in fact, most students of China in the 1930s would be in agreement. But our acceptance of this viewpoint, I must candidly state, has to date been based less on scholarly research on the decade 1927-37 than on our knowledge of how the next installments in the serial story of the Kuomintang turned out. Unfortunately, Professor Tien's study only marginally contributes to filling the scholarly gap. He states, and I acknowledge, the unavailability of the Kuomintang archives, but he has barely touched the rich contemporary periodical and newspaper sources. The two chapters on central and provincial revenue and expenditure contain little new factual information and are analytically quite primitive. I was at a loss at times to comprehend the relevance of the extensive tables in the long chapter on "the

social composition and turnover of provincial elites." This is, in sum, a flawed effort that, however, asks the right questions.

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HOLMES WELCH. *Buddhism under Mao*. (Harvard East Asian Series 69.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xviii, 666. \$16.00.

With the publication of this volume, Professor Holmes Welch, research associate in East Asian studies at Harvard, has completed a series of three books on Buddhism in modern China. The author admits candidly that it was more difficult to write this volume than the two previous ones, *The Practice of Buddhism, 1900-1950* (1967) and *The Buddhist Revival in China* (1968), because sources of information have been scant and have grown steadily scarcer and any statement concerning Communist China is bound to be controversial. Inasmuch as the history of Chinese Buddhism since 1949 involves millions of people diffused over a vast area with many regional differences, it requires unusual effort for any outside researcher to ascertain which, and to what degree, Buddhist institutions and practices have changed. In this respect the author should be commended for having combed through a staggering amount of relevant data, including those that were based on his personal interviews. The author says cogently that "ultimately a book like this has to depend on using odds and ends of inadequate data to make judgments, rather intuitively, about what *probably* happened" (p. viii). While we are in no position to judge the adequacy of Welch's perception, we learn a great deal about contemporary Chinese Buddhism from his lucid and insightful accounts.

Welch tells his narrative under ten major headings. In chapter 1, "A Policy Emerges," he traces the development of the official policy of the Chinese Communist party regarding Buddhism by sorting out the ambiguities and inconsistencies involved in the attitudes of the Religious Affairs Bureau toward the journal *Modern Buddhism* (*Hsien-tai fo-hsüeh*) and toward the Chinese Buddhist Association. He

finds that the official policy, at least until the Cultural Revolution, was to "protect Buddhism, while at the same time keeping it under control and utilizing it in foreign policy." How this policy was implemented is told in chapter 2, "The Decimation of the Sangha," chapter 3, "Making Monks into Good Citizens," and chapter 4, "The Reform of Monastic Life." The overall effect of this policy is succinctly characterized as follows: "The trend towards reorganization was offset by the trend for the number of resident monks to decrease. At many places there were simply too few people left to run anything but a museum. In any case, regardless of how monasteries were organized—whether they were headed by an abbot or a prior or a committee—real control had passed out of the sangha to the government, at first to the civil affairs bureau, later to the religious affairs division. Monasteries had lost their traditional autonomy" (p. 139).

The author makes a similar observation in chapter 5, "Preserving Buddhist Culture." After discussing the regime's claim for the conservation of Buddhist art and architecture, he states that "the art historian will be pleased that so much restoration was done, but the historian of religion may be less impressed. What interests him is not how many temples were restored but the degree to which they remained in religious use" (p. 168). Chapter 6, "Buddhism in Foreign Relations," shows how the Peking regime attempted from 1952 to 1966 to use Buddhism in order to influence public opinion in the neighboring Buddhist nations in Asia. That this policy was abruptly reversed on the eve of the Cultural Revolution is attributed by the author to "a feeling by Mao and others that the use of Buddhism in foreign policy had been more trouble than it was worth" (p. 228).

In the following chapters—"Suppressing Buddhist Opposition," "Interpreting Buddhist Doctrine," "The Laity," and "The Individual Buddhist"—the author portrays a bewildering picture of crisscrossing influences between government programs and the residual strength of Buddhist beliefs and practices on the part of the laity and the clerics. Of special importance is "the resourcefulness of Buddhists in trying to survive" (p. 266), even in the midst of almost insurmountable odds. Accord-

ing to Welch's account in chapter 11, "The Cultural Revolution and After," no news items were printed in the Mainland press after August 1966 concerning the Chinese Buddhist Association or the activities of monks and lay devotees. The author concludes the last chapter, "The Future of Buddhism in China," with the mildly optimistic opinion that elements of Buddhist belief and practice will remain a part of the "spontaneous religiosity" of the Chinese people in the years to come.

Readers will no doubt take issue with the author's interpretation of various events and the motivations of heroes and villains, both in the government and in Buddhist circles, mentioned in this volume. Nevertheless, all students of Buddhism and Sinology are greatly indebted to Professor Welch's careful and thorough analyses of the dramatic pages of Buddhism in contemporary China.

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JOYCE C. LEBRA. *Jungle Alliance: Japan and the Indian National Army*. (Asia Pacific World Library.) Singapore: Donald Moore for Asia Pacific Press. 1971. Pp. xiv, 255.

Jungle Alliance is a welcome addition to our understanding of the significance of the Greater East Asia War, which, according to an orthodox interpretation, was simply a Japanese version of the Western capitalist pattern of imperialism. Lebra has challenged such a notion.

The book is a product of painstaking research spanning ten years during which Lebra traveled to India, Japan, and Southeast Asia. The result is an authoritative account of "the interaction between Japan and the Indian independence movement in Southeast Asia." She narrates the story with objectivity.

The uniqueness of this study is that out of the narrative emerges a young Japanese major, Fujiwara, of the legendary *F Kikan*, who played a dominant role in the creation of the Indian National Army (INA). Lebra skillfully narrates the importance of Fujiwara in the making of South Asian history and provides a much-needed study of a remarkably individualistic policy entrepreneur and of the organization (*kikan*) he headed. Equally unique is the fact that India remained peripheral in Japanese military strategy, which was never

devised to push the boundaries much beyond Burma. Yet, this marginal interest in India produced the most charismatic revolutionary of all wartime Asian nationalists, Subhas Chandra Bose, who led the INA against British troops. Lebra describes well the impact of Bose upon such Japanese political and military leaders as Premier Tojo, army chief of staff Sugiyama, and the Burma area commanding general Kawabe.

Bose, as depicted by Lebra, was a single-minded revolutionary for whom the alliance with Japan was only a means for achieving the goal of Indian freedom. Nothing deterred him from this objective. This, together with his tragic death en route to Russia three days after the end of the war, is probably the reason he became a legend that still lives on in India.

Jungle Alliance supplements and complements K. K. Ghosh's *The Indian National Army*, published in India in 1969. Together they may well remain the definitive study of the INA. This is a fascinating study from which students of Japanese and Indian history may gain great satisfaction.

I should note the absence in the bibliography of volumes 8 and 9 of Yomiuri Shimbunsha's *Showashi no Tenno* (*The Emperor in Showa History*) (1969), which give detailed eyewitness accounts of Bose's submarine trip to Sumatra from Germany, of his fighting in the front during the Imphal Operations, and of his death in an airplane crash.

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H. PAUL VARLEY. *Imperial Restoration in Medieval Japan*. (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1971. Pp. xi, 222. \$8.00.

This is a study of the Kemmu Restoration (1333-36), first, as a crucial development in the shift of power from civil aristocrats to soldiers and, second, as a topic of historical reflection from that day to the present. Professor Varley has utilized the findings of Japanese historians, read extensively in contemporary chronicles, and supplied new information. His treatment of institutional changes made during the Kemmu Restoration and his discussion of the *Jinnō Shōtō Ki* (*Chronicle of*

the True Line of Sacred Emperors)—written between 1339 and 1343—are of special interest. But as valuable as these contributions are, they do not do much to help us understand the Restoration phenomenon or to see patterns of change in Japanese historical thought.

Professor Varley appears to think of the Kemmu Restoration as no more than a "final bid by the court nobility to assert its claim to the rulership of the land." Conflict between the old and new elites was certainly there, but surely it was the deep-seated, highly institutionalized beliefs concerning the sacred emperor that led each contender to fight for imperial sanction. A rigorous investigation of the throne's symbolic significance might indicate that Emperor Go-Daigo failed to gain control of the state because he did not make a sharp distinction between his sacerdotal functions and the handling of administrative affairs. Under the influence of Chinese political thought, and of Japanese precedents established when Chinese models were being followed, he may have strayed too far from the imperial way when he tried to be prime minister and shogun as well as emperor.

In comparing the *Jinnō Shōtō Ki* with the *Gukanshō*, which was probably written in 1220, Professor Varley does not move much beyond the observation that the former rejected the *mappō*-based pessimism of the latter and concentrated on "the true line of sacred Emperors." By delving into the intellectual and religious content of such terms as *seiri* and *dōri*—terms that represented different convictions about the sacred dynamics of history—he might have helped us to understand better the different positions of these two classics in the evolution of historical consciousness.

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ROGER F. HACKETT. *Yamagata Aritomo in the Rise of Modern Japan, 1838-1922*. (Harvard East Asian Series 60.) Cambridge, Mass.: Harvard University Press. 1971. Pp. ix, 377. \$12.00.

This book undertakes the formidable task of a "life-and-times" style biography of one of the most influential figures in modern Japanese political history. As a designer of the Meiji political order, Yamagata is known primarily

for his leadership in organizing a universal conscription system, forming a modern army, and establishing a semiautonomous position for the military within the government. He was particularly influential, too, in his role as builder of local government institutions, using the Home Ministry, which controlled local administration, as a powerful instrument for popular mobilization and for integration of the citizenry into the national political processes. With personal power thus rooted in the military and civil bureaucracies, he was a force to be reckoned with in national politics for nearly half a century.

His biographer, therefore, must cover a good deal of territory. As if so large an assignment were not challenging enough, he also must confront the peculiar difficulties of writing Japanese biography. Japanese leaders often appear flat and colorless as a result of paucity of biographical material, the murkiness of decision-making, and our uncertainty over how to analyze Japanese personality. Professor Hackett, for example, is handicapped by a lack of information about Yamagata's first thirty years and can find little to relate beyond the bare outlines of his early life. We get little feeling therefore of how Yamagata's experiences during these formative years conditioned his attitudes during the remainder of his career. Once beyond the Restoration, the author has more biographical material and the narrative picks up steam. With admirable clarity he surveys his subject's role in the political history of the next fifty years, making imaginative use of Yamagata's poetry to enliven the account.

Yet, as able as Hackett's narrative is, the book ultimately disappoints—only in part owing to difficulties inherent in Japanese biography. The greater disappointment is that the scholarship of the 1960s is not digested into the substance of this work. Professor Hackett does not speak directly to the major historiographical issues that have been hotly debated in the decade preceding the publication of his book. These issues involve interpretation of the Restoration, the making of the constitution, and the rise of political parties, in all of which Yamagata was prominently involved. The reader can guess what the author's views on some of these issues may be,

but he will wish that Professor Hackett had brought his understanding of political history to bear directly on these debates.

KENNETH B. PYLE

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F. G. NOTEHELPER. *Kōtoku Shūsui: Portrait of a Japanese Radical*. New York: Cambridge University Press. 1971. Pp. x, 227. \$16.00.

Recent biographers of Japanese figures have turned as often as not to radical intellectuals—dissidents who advocate direct action—rather than to members of the ruling elite. Doubtless the attraction of the radicals derives from their colorful careers and their penchant for committing their innermost thoughts to paper, both of which facilitate the writing process. Moreover, the vogue for these romantics, especially the martyred among them, in Japan today gives them a contemporary relevance. Professor Notehelfer's distinguished and well-documented study of Kōtoku Shūsui is a worthy addition to this genre. The Notehelfer biography is particularly notable for its delineation of the striking personality of his tormented subject. In these pages Kōtoku lives and breathes as a whole man in his Meiji environment while making his intellectual odyssey from *shishi* loyalism—by way of liberalism, socialism, and anarchism—to advocacy of imperial assassination.

Kōtoku's commitment to revolutionary action had its roots in traditional values, the author asserts, and he makes his case. It was political violence born of admiration for the swashbuckling *shishi* of the era before the Meiji Restoration. Because Kōtoku regarded them as successors to the *shishi*, assassins of the later Meiji era regularly won praise from him, sometimes fulsome praise in ode form. A late infusion of support for direct action from Western radicalism, from the Wobblies, and from others, merely reinforced Kōtoku's violent tendencies but pointed to the head of state of an exploitative society as the prime target.

This is fundamental—that the Meiji emperor should be considered a "murderer" deserving assassination by one whose early inspiration came from the loyalists in the Restoration of this same Meiji emperor. It seems to me, if I read the text correctly, that

the author does not altogether make his case that Kōtoku's thought—apart from his tactics—developed out of a “restructuring of traditional values” (p. 3). Notehelfer's own evidence suggests a massive injection of Western radical thought to bring Kōtoku to advocacy of regicide. The reservation is minor. The book, in fact, provides a splendid, incisive record of the constant revision of his political thought by Kōtoku, a practicing journalist, as ideas flowed to him from foreign publications and through residence in the United States. His march toward ever more radical programs for social justice coordinated with his deepening political and personal frustrations.

Kōtoku is here—warts and all. He wrote on behalf of social justice, yet dressed as a dandy, kept his distance from steerage passengers when aboard ship, and generally shunned the illiterate masses whom he found distasteful. He expressed compassion for people in the abstract but was notably cruel to those, particularly women, closest to him. Finally, he spent a career in opposition to the Meiji leaders, yet at the end allowed himself to be bought off by the home ministry to cease radical activity and study away from Tokyo.

For his link with the high treason case, an alleged plot to assassinate the Emperor Meiji, Notehelfer treats Kōtoku as neither “first martyr” nor “arch traitor.” In this dispassionate volume, Kōtoku comes through as one who rallied conspirators to the cause of imperial assassination with inflammatory words, only to back away from the plot as it matured. He returned, nevertheless, to die in style, unflinchingly, denying the accusations not at all, after a secret and controversial trial. The author suspects that the transcript of this trial may still exist, unreleased. Unless this document comes to light, the present biography will continue to be the definitive English-language study of Kōtoku.

SIDNEY DEVERE BROWN

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KATHARINE SANSON. *Sir George Sansom and Japan: A Memoir*. Tallahassee, Fla.: Diplomatic Press, 1972. Pp. xvi, 183. \$15.00.

First, it should be said that this is a very stylish book, definitely a credit to George Lensen, the

Diplomatic Press, and the Irish printers who handled the production. And it is charmingly written. The cultivated style for which the late George Sansom was justly famous shines through, not only in the excerpts from his own writings, but in the general narrative as presented by his widow, the author. Despite her disclaimer, it is entirely worthy of Sir George.

The content is fascinating. Japan specialists of our present era will be enthralled by the story of the career of this great amateur, who in a very real sense provided in his *Japan: A Short Cultural History* (1931) the basic text from which most of these specialists got their start. It is clear that almost from the moment of his arrival in Japan in 1904, at the age of twenty, as a neophyte in the British consular service, Sansom began to soak up the culture that went into his later books. The encouragement he received from Basil Hall Chamberlain, Sir Charles Eliot, Langdon Warner, and other Western scholars is acknowledged, as is his indebtedness to many Japanese scholars with whom he spent hours discussing the fine points of his studies. The course of those studies, which resulted in the production of *An Historical Grammar of the Japanese Language* (1928), the aforementioned *Cultural History*, *The Western World and Japan* (1950), and *History of Japan* (1958–61), as well as several short pieces, show him persisting through all manner of distractions, consular reports, diplomatic assignments, the Pacific War, financial worries, and stomach ulcers to complete them. The story will warm the heart of any scholar.

But this is not merely a testimonial to scholarship. Historical events that Sansom lived through, but never wrote about in his books, the events of the 1930s and forties that found Japan on the road to war, into it, and through defeat and occupation are seen here through his eyes for the first time. His observations contain information useful to a variety of historical specialties. His recollection of the conversation between George Bernard Shaw and General Araki in Tokyo in 1933 is the first of a number of insightful commentaries on the Japanese militarists. His private discussion with Shigeru Yoshida (1940) provides an excellent basis for understanding Yoshida's role as postwar Japanese premier; Sansom's brief comments from Singapore (1942) explain for military history the

gamble on the weather taken with the *Repulse* and *Prince of Wales*, a gamble lost.

Regarding the occupation of Japan, which concerned him as a member of the Far Eastern Commission, Sansom's views were definitely conservative. He admired General MacArthur, was "not sure that the Americans realize what they are doing in their enthusiasm for freedom," worried about the "Communitic leanings" of "many of the young officers at G.H.Q.," and thought there was not "a shred of evidence" that the Zaiatsu had "deliberately and willingly collaborated with the military." Though he felt that "the present liberals" (like Shidehara) were "too broken by past events" to be effective, he distrusted Japan's intellectuals as "critical of occupation policy" and "fundamentally anti-white." Certainly he was glad that his prewar friend Yoshida Shigeru emerged to lead Japan toward recovery while maintaining conservative moorings (chapter 12).

Since Sir George singles out Professor Ayusawa Iwao as an example of the "anti-white" Japanese intellectual (p. 152), it seems important for me, who knew Ayusawa fairly well, to point out that he was in fact a true internationalist, a "christian," in the broadest and best sense of the word, who devoted his life to international causes, including in his later years the establishment and development of Japan's great International Christian University at Mitaka. That Sansom could not appreciate Ayusawa is an indication of a flaw in himself rather than in Ayusawa. Sansom, for all his appreciation of the intercultural world, was to some extent patrician, impressed with titles, "class," and Anglo-American leadership in international affairs, which Ayusawa realized was too narrow a base for a world system.

Hence we may conclude with the observation that George Sansom did brilliantly what he did, which was cultural history, but it is probably just as well that he confined his writings on international politics to his diary and private papers, for his judgment on these was by no means infallible.

HILARY CONROY

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ROBERT A. SCALAPINO and CHONG-SIK LEE. *Communism in Korea*. Part 1, *The Movement*; part 2, *The Society*. (Published under the auspices of the Center for Japanese and Korean Studies,

University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xxi, 685; xi, 688-1533. \$25.00; \$30.00.

The authors of this definitive study believe it extremely doubtful that a resurrected Karl Marx would view the Democratic People's Republic of Korea as a state drawn from his inspiration or as a true socialist society. Even Lenin might find it difficult to accept the creed of communism in North Korea and the structure of Kim Il-sŏng's polity, although Lenin would be more understanding than Marx.

Indeed, the present North Korean system was by no means inevitable, or even natural, for the society. Once it was implanted by Soviet power, however, and then evolved on its own, the system became a crucial variable and caused an ever-widening divergence between North and South. A few years ago Professor Glenn Paige wrote an essay for a conference on modernization held in South Korea and similarly predicted that the political variable would be of vital importance to such emerging societies. As the Scalapino-Lee study of the two Koreas illustrates so graphically, "that variable is not mechanistically determined by the particular stage of socioeconomic development in which the society finds itself, Marxian theory notwithstanding" (p. 1316).

Such reasoning practically guarantees that this monumental two-volume work will be dismissed out of hand by some scanners of "new leftist" persuasion, by those "new monolithists" who expect only similarities throughout the so-called third world, and by those with an uncritical predilection for any "communist" regime. There are other reasons. For example, the authors carried through painstaking research in prewar Japanese documents, early journals, rare leaflets, obscure newspapers, and elusive mimeographed items; they also relied heavily (for part 2) on in-depth interviews with thirty-four former North Koreans (that is, defectors) conducted in the 1967-70 period. These were utilized only after the authors conscientiously checked them against biographies, Comintern documents, official publications, and English-language material issued by the Foreign Language Press in Pyongyang. Nonetheless, the importance and validity of such data will be challenged by some readers.

The forthright stance, rather than the credentials, of the authors will be questioned

by others. Robert A. Scalapino is professor of political science at the University of California, Berkeley, and author of *The Japanese Communist Movement, 1920-1966* (1967) and numerous other Asian studies. Chong-sik Lee is associate professor of political science at the University of Pennsylvania and author of *The Politics of Korean Nationalism* (1963). They use an objective, balance-sheet approach. Nonetheless, they present devastating profiles—of a movement plagued by factionalism and failures; of a society proclaiming dedication to internationalism and, in fact, inspired by an ultranationalist, exclusivist, and vaguely racist principle (*chuch'e*, “the principle of making indigenous need and experience the essential criteria, of implanting *self-sufficiency* as the central theme of the Korean revolution”); of a reconditioned guerrilla-hero leader who administers a twentieth-century monocracy with a Stalin-like cult of personality, existing (incongruously) alongside an elaborate institutional structure and refined creed of collectivism; of a modern party elite who, if they are not drawn from the *yangban* (gentry), neither are they from the ranks of the urban proletariat; and of a state dedicated to the creation of a new socialist human being and to the salvation of man, but in which concern about men steadily diminishes.

Beyond controversy, this is a pioneering work in the field of comparative communism, now in its infancy. It makes frequent comparisons with other communist systems, particularly in Russia and China, with which North Korea has been closely associated. The authors make a case for a more accurate typology of communist systems, including delineation of their common and particular features. The Democratic People's Republic of (North) Korea has been and remains by self-definition “communist.” What does this mean?

The study demonstrates that Korean “communism” has been indelibly stamped by traditional (Korean-Confucian) political culture, warped by its peculiar (Japanese) colonial experience and evolution in (Manchurian) exile, nurtured under alien (Russian) domination and salvaged by alien (Chinese) military aid, and tested in (Korean civil war) emergency. “In the final analysis, it is the dynamic interaction among Communism, emergence, and tradition that provides the fullest context in which the

politics of the society operate” (p. 1306). In the present order of priorities and in the means used, the authors conclude that the Korean Communists reveal themselves to be leaders of an emerging society first, Marxists second. Kim and his cohorts have not seen Marxism as the culmination of the modernization process, but rather have sought to make of it an instrument of modernization and in this effort have basically altered Marxism.

To the specialist on East Asia and to the depressingly few experts on Korea, the two-volume work represents a staggering challenge. In sheer volume of data sifted and presented, as well as in comparative insights, it is difficult to imagine that this study will soon or easily be matched. The first eight chapters (part 1) present a historical, developmental treatment, with emphasis on the origin and relationships with other Asian communist movements. The final seven chapters (part 2) turn to topical, social-science oriented analysis, with emphasis on the evolving character of the political elite and most especially on the emergence of Kim Il-sŏng to a position of absolute power and evolution into an institution. Attention is paid to organization, the role of ideology (my favorite chapter), the functions of special groups (for example, the cadres), the status of the military, the economic structure, and performance. Most enlightening are the descents from the abstract to microlevel vignettes of the actual life-styles of the intellectual, artist, student, party cadre, soldier, peasant, and worker.

To the most critical and persistent expert, the two volumes offer rich rewards in massive documentation. There are detailed maps, numbered (twenty-nine in part 1, seventy-two in part 2) and unnumbered tables, and appendixes. These include the constitution, party rules, lists of Central Committee members (1948-70) ordered by rank, cabinets (1948-70), charts of selected commissions, ministries, and local governments, as well as statistics on prices, wages, and monthly budgets. The authors present their interview schedule and a complete bibliography (subdivided into Korean, Japanese, English, Russian, and Chinese sources). The index itself is a detailed forty-one-page pamphlet inserted between parts 1 and 2.

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New Brunswick

HAZEL KING. *Richard Bourke*. New York: Oxford University Press. 1971. Pp. xv, 312. \$15.50.

Sir Richard Bourke, governor of New South Wales from 1831 to 1837, was probably the most attractive and popular governor in Australia before 1850. Dr. King has written an outstanding biography that is a model of compression. She exploited the fine collection of Bourke papers in the Mitchell Library, Sydney, and ferreted out private collections in Australia, England, and Ireland. Her skillful use of quotations gives us a vivid sense of the man and his family. Newspaper files and official government documents were exhaustively examined, and the issues of the time have been clearly presented and woven into the thread of the narrative. She is cautious, fair, and balanced in her judgments. The result is a full-length biography with fresh information about Bourke's early life, education, military career, home life in Ireland, and his governorship of the Cape of Good Hope, 1826-28. Less than half the book covers his administration in Australia.

It is now apparent that Bourke might have risen high in the British Army. Between 1798 and 1806 he showed great promise. He fought with Wellesley in the Peninsular campaign, but put his family before his career and dropped out of public life between 1808 and 1812 to guard his wife's health and to help the treatment of his oldest son's hydrocephalus. Returning to Spain, he served in a quasi-diplomatic role, then retired to Ireland from 1815 to 1825 to live the life of a country gentleman.

Coping with the imperial problems of the Cape of Good Hope prepared Bourke for handling similar ones in Australia: disputes over the press, the bickerings in the executive and legislative councils, native policy, taxation, the consistent parsimony of the treasury, the interference of the Colonial Office, and others. Dr. King rates Bourke a success and deserving of the assignment to Australia.

Arriving in Sydney, Bourke's affiliation and his dependence on the new Whig government in England are stressed by Dr. King. We also learn much about Bourke's Australian friendships, especially his closeness to the Chief Justice, Francis Forbes. Dr. King brings out the serious thought given by the Colonial Office to the immediate abolition of penal transportation. The chapter on the aborigines is too short,

though the point is clearly made that Bourke considered the problem insoluble. He did not understand the natives, a surprise as he had been much more successful with them in South Africa. More could have been written on his success with religious toleration and his failure to introduce public education. Dr. King accentuates the weak support Bourke received from London, which eventually caused his resignation. Both his church and education policies were hamstrung by Glenelg's indecision. However, Bourke received support for his idea of permitting pastoral expansion by granting licenses, renewable each year, to squatters on Crown lands outside the boundaries of location. Indeed, his six years in Australia were ones of economic prosperity, particularly in the wool trade. His immigration policy of free settlers was also successful.

After Bourke retired to Ireland he refused other posts, such as the governorship of Jamaica. Dr. King's thorough study has illuminated both a fascinating human being and his times.

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J. A. LA NAUZE. *The Making of the Australian Constitution*. (Studies in Australian Federation.) [Carlton:] Melbourne University Press; distrib. by International Scholarly Book Services, Portland, Ore. 1972. Pp. xi, 369. \$20.00.

To the student of government it is obvious that there have been very few unions of states or provinces reached by popular assent that have endured. The United States is the oldest and Switzerland the only example outside the English-speaking world. Historically the great states of the ancient and modern world have grown by military conquest, and their strength and endurance have depended on the strength of the central government of the conquering power. Another universal truth seems to be that such federations come into existence only in the face of considerable popular opposition and have usually been achieved under pressure from the economic rivalry or military threat of outside powers. In the case of the United States and Canada the strongest opposition came from the smaller units—Rhode Island and Prince Edward Island are examples—but in Australia, New South Wales, the largest and oldest prov-

ince, was the most difficult to convince. Politicians in Sydney, wishing, no doubt, to claim leadership in the commonwealth that they knew was coming, worked hard as framers of the new constitution, but they never wholly converted their constituents.

La Nauze does not explain this divergence from what might be called the norm of federal movements, but he makes clear that the movement as a whole set a record for the length of time it took Australians to make up their minds that federation was a necessary step in their political evolution. Fifty years went by—in which many of the wisest statesmen recognized the step as inevitable—before the Commonwealth of Australia was proclaimed in 1901. Australia, the “quiet continent,” did not suffer from the pressures that had harassed the United States and Canada.

La Nauze's volume is a meticulous and fully documented account of the conventions at Melbourne, Adelaide, and Sydney, and also of the discussion at the Colonial Office in England where the Colonial Secretary, Joseph Chamberlain, was chiefly concerned lest the right of appeal to the judicial committee of the Privy Council in England from the High Court in Australia be abridged in cases involving the British investors of capital. The book will be invaluable as a reference work for Americans teaching British imperial history, but it cannot be recommended for light reading. Fortunately the last chapter, which sums up some of the conclusions reached earlier, is good reading and of interest especially to us because of the pages dealing with the influence of the American Constitution and American constitutional government as the Australians understood it. The chief source for its study was James Bryce, *The American Commonwealth* (1888), which “lay on the Table” for all to consult during the meetings in 1897 and 1898. The document sent to England, which is one of many useful appendixes, does not very closely resemble the American Constitution as we know it, being much more diffuse in wording and much more flexible in the powers given to the Australian Parliament to make changes in some fundamental provisions. Even so, La Nauze ends with the criticism that the constitution should have provided for total reconsideration and revision at intervals. Apparently he feels that if it were

rewritten today, a good deal of it could be omitted.

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UNITED STATES

DAVID M. POTTER. *History and American Society: Essays*. Edited by DON E. FEHRENBACHER. New York: Oxford University Press. 1973. Pp. x, 422. \$10.00.

A quotation from the first of these essays, “Explicit Data and Implicit Assumptions in Historical Study,” could stand as a fitting epigraph to the book: “The evaluation of significance may be a matter of sagacity and applied experience which cannot be taught as method. When we encounter this sagacity in politics, we call it statesmanship, and we do not for a moment suppose that students can be trained in school to be statesmen. When we encounter it in historical studies we are likely to call it ‘an awareness of the historical process.’”

The rest of the book exemplifies this awareness, along with Potter's conviction that “the tasks of the historian must change as our conception of the problems and issues of society changes, and we must be prepared for the fact that the historian's work will not lie in those areas where his methods make him feel most at home but in those where society's need for an understanding of its past is most acute” (“Tasks of Research in American History”).

This is an invaluable and thought-provoking book, one from which the general reader as well as the historian can profit. The title, it should be understood, has reference to the structure as well as the subject matter of the book: nine essays on history and seven on American society. Of the sixteen essays eleven were previously published, but some of them in relatively obscure journals. The most familiar is probably “The Historian's Use of Nationalism and Vice Versa,” in which Potter used the experience of the Civil War as the focus for a wide-ranging exploration of the relative importance of cultural differences, historical process, and conflicting interests to the phenomenon of nationalism. The essays on history include, in addition to those mentioned above, one in which Potter, while paying his respects to social science, argues that its methodology

cannot meet the historian's need to form judgments, to deal with ideas and influences, and to consider heterogeneous elements. A short piece on the problem of large-scale community formation serves as a kind of preface to the familiar essay on nationalism, and the first part of the book is rounded out by a critique of the Turner thesis, reprinted from *People of Plenty*, and essays evaluating each of three historians: C. Vann Woodward, Richard Hofstadter, and Roy F. Nichols.

The essays on American society turn largely on the question of the American character and in nearly every case advert to the conflict between individualism and equality, the latter being "conducive to conformity rather than to freedom, since it places the stigma of arrogance upon any man who ventures to set his personal judgment against the judgment of a majority of his equals." The previously unpublished essays include one that is probably the most important and easily the most arresting in the entire book: "Rejection of the Prevailing American Society," a lengthy yet all too brief preview of a major historical study of alienation in American life that Potter had projected before his death in 1971.

Dissent, Potter argued, had come to find outlet in confrontation and disruption because of the decline of sanctions against such behavior. Down to early modern times the main sanction had been simply coercion, and aggressive impulses had to find objects other than the institutions of society. After the American Revolution, however, coercion had been replaced by the impulse to conformity, which meant conformity mainly to WASPish values in society. But by the 1920s and 1930s those values had suffered crippling blows, most conspicuously in the failure of Prohibition and the social trauma of the Great Depression. But perhaps most crippling of all was the rise of a new class, the pedagogues of academia who became intellectuals and "set busily about laying their axes to the mythic underpinnings of the American identity." By the 1960s the defenders of society were guiltily aware of its failure to fulfill its own ideas and disarmed also by "broad, absolutist, and somewhat indiscriminate ideas of the right of dissent." Society even accorded a kind of sanction to disruption.

By way of summary, Potter's argument goes

something like this: all societies have a quantum of discontent, some of it rationally derived, but not necessarily commensurate with either the degree of injustice or the degree of social protest, since discontent in the past has found other outlets, like revival meetings. Finally, the private and individual sources of much discontent have traditionally been ignored by historians, although they may be widely prevalent and may indeed have arisen from historical changes. The last part of the essay, therefore, focuses on problems of individual identity and roles, more specifically on the male sex role.

The growing difficulty of male identification with the father, as Potter noted, has been widely recognized. Less widely recognized has been the decline in the role of the play group of pre-pubescent males, often celebrated in such classics as *The Adventures of Tom Sawyer* or, in its more cruel and tribal aspects, *Lord of the Flies*, but less often appreciated as a functional institution in the development of masculine identity. Somewhere along the way the group, too, has lost its authority, leaving large numbers of males confused about their identity and role in society and filled with a dislike for authority, whether exercised by themselves or others, and turning elsewhere for the fulfillment of personal needs—to identification with symbols of rejection, to the drug culture, and the hippie culture.

This essay, along with pieces on "The Roots of American Alienation" and "Social Cohesion and the Crisis of Law," offers tantalizing glimpses of what the larger study might have been. The focus on male roles in "Rejection of the Prevailing American Society" is matched by a focus on female roles in "American Women and the American Character" (first published in 1962), which has achieved the status of a minor classic in its field. Taking off from the point that "our social generalization is mostly in masculine terms," Potter noted that many such generalizations—about the effect of the frontier, regimentation of labor, specialization, sedentary occupations, the growth of other-directedness—have little relevance to the lives of women, whose experience in some cases has been opposite to that of men. If women have gained in a labor system in which biceps are no longer at a premium, society has remained unable to

"make her career aspirations and her family aspirations fit together as they do for a man." And those who have become housewives have encountered new frustrations—frustrations in being the only workers who do not get paid, in their new economic role as consumers rather than processors, and in their new social role of keeping up with community activities.

The book is so full of substance that no review can possibly touch upon everything of value that it contains. It reflects, moreover, the author's constant dedication to rational judgment, to live issues, and to a simple felicity of style happily free from polemics and jargon. Among other things, Potter was no mean epigrammatist and the editors of the next *Bartlett* will do well to dip in here before they go to press. A few pertinent samples: "An adversary need not be a mortal enemy." "If hypocrisy is the tribute that vice pays to virtue, permissiveness is, to some extent, the tribute that authority pays to the principle of equality." "The power to disrupt is the power to discredit. And in public affairs, the power to discredit is the power to destroy." "The spirit of the commune is . . . an unstructured diffuse sense of 'love' toward everyone in general and no one in particular." "The myth is to the society almost what a sense of personal identity is to the individual." "But history often mocks logic."

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SYDNEY E. AHLSTROM. *A Religious History of the American People*. New Haven: Yale University Press. 1972. Pp. xvi, 1158. \$19.50.

This book is a monument not only to its subject and to the ample support of the Yale University Press but also to the prodigious enterprise, the scope of imagination, and the range of interest and empathy of its author. It is an attempt to write the history of American religious life in the four-hundred-year period from the time when the Reformation was more or less established until the 1960s, with a brief but poignant coda suggesting another spiritual era sloughing toward Bethlehem to be born, as Yeats had foretold. At times one may be

deluded, by Ahlstrom's sedulous attention to the detailed transition from decade to decade, into assuming that this book is one more, perhaps a more grandiose, celebration of the triumph of religion. But Ahlstrom's schema, though informed by his grand conception of an epoch of religious life that was born, developed, and then imperceptibly weakened, is not obtrusive. In manner, he is closer to Neander than to Hegel. In spirit, he is much too aware of the cunning twists that history takes to allow his general view to obtrude upon his close reading of the ironies and ambiguities of cultural development. To employ an impious comparison, for an American, he looks at history more in the manner that Lincoln regarded the Civil War than in the modes of Horace Greeley, Wendell Phillips, or Bishop Lynch of Charleston, South Carolina. It is characteristic of his perception of past, present, and future that he can conclude a chapter on Roger Williams's Rhode Island by saying that that experiment "seems to illustrate in an almost tragic way the political corollary of a dictum often voiced by historians of science, that premature discoveries are uninfluential."

But he does take his periodization seriously. He does believe that the 1960s mark the inauguration of a new period. Not only has the Roman Catholic Church largely abandoned the triumphalism that since Trent dominated its thought, but Judaism is back in the wilderness, and Protestantism is increasingly polarized between Jesus-freaks and God-squads and increasingly tempted by an intellectual dosage of speed and tranquilizers in its holy place and holy hour.

In writing of an epoch in (primarily) Western religious experience Ahlstrom refuses to be parochial. Though he scants the Reformers more than I should have liked he wisely gives more space to Kant than to Bushnell: and he acknowledges that the end of Victorianism marked an end of certainty for almost everyone in the West. Nor is he seduced into writing church or denominational history. Where the church or the denomination is a real force he discusses them, but he sees no need to devote unusual time to organizational setting-up exercises; intelligently he is willing to make exceptions, as with his treatment of Christian Science, which he finds more notable for its

organizational brilliance than for the character of its ideology.

Somewhat more problematically, Ahlstrom defines quite narrowly what he means by "a religious history of the American people." He gives little attention to the way in which economic, political, cultural, and social life in this country has been shaped by religious presuppositions, if not dicta. And though in the introductory sections of each of his more than sixty chapters he sketches shrewdly and succinctly the milieu of the religious life he will be concentrating on, these are obviously not at the center of his attention; it would be, accordingly, foolish to nit-pick at some of these opening paragraphs or pages. His stake, and the reader's, is in what follows. And what follows is very, very good. Another principle of Ahlstrom's book is that he unhesitatingly recognizes that no more than an approximate chronological order can be adhered to. Individual readers might want to read about Hasidism in the context of Kant and the romantic movement; and others may be surprised to find Mary Baker Eddy so many pages removed from discussions of the "transcendental strain." But it is exceedingly unwise—as I found in preparing this review—to conclude that Ahlstrom is leaving something out. One may quarrel with his architectonics but not with his close attention to the many mansions in American religious life. One's own blueprint might be different, but be assured that Ahlstrom has provided a room for virtually every view that can be conceived of.

A consequence of this intellectual ecumenicism is that even in a volume of more than one thousand pages the author must abbreviate. Aware as most of us are of Ahlstrom's deep knowledge of New England theology, or of Mercersburg, or of the strands of Lutheran thought we may feel at times that he is compressing his knowledge into the constrictions of a new Hastings Encyclopedia—in one volume! But one wants more on a subject for the best of all reasons: that Ahlstrom has above all been judgmental, discriminating, and persuasive in what he does have room to say. At the same time he is empathetic with what foreign observers have sometimes regarded as a zoo of religious curiosities. Take, for example, his discussion of neo-orthodoxy in American Protes-

tantism. After some incisive remarks about the Niebuhrs, he notes the breadth of the attitude, which embraced orthodox Presbyterians and Pentecostals; all joined in "an assault on both the great romantic doctrine that the religious and/or moral consciousness provides the proper starting point of theology. . . . The genteel tradition must go; metaphysics cannot do duty for the revealed Word of God." "A situational-contextual 'love-ethic' became the positive part of a widespread critique of legalism and code morality"; "because neo-orthodoxy did not rest its ultimate faith in human arrangements, it could bear—or even advocate—the shaking of cultural foundations." Neo-orthodoxy, in contrast to fundamentalism, showed great respect for the "scientific, scholarly, and artistic achievements of men." It also put great stress upon "the Church," and especially "the prophetic church that would recognize its continuity with the New Testament community, and, therefore, its distinctness from the world in which it proclaims the Word and to which it ministers." The early Fathers, the councils, and, above all, the Reformation theologians "also gained new currency. . . . They were taken seriously rather than dismissed in the liberal manner as outmoded stages in the evolution of pure religion."

No one will find everything in this volume that the title and the richness of the text leads one to hope for. I, for example, would have liked a bit less on anti-Catholicism and more on the inner tensions and resolutions, however inchoate, of American Catholics. And, curiously, Ahlstrom, while profoundly sympathetic with what James might have called the religious "impulse," takes its existence for granted and does not probe deeply into the changing psychology of its provenance. But let me repeat: he is no muckraker; in a book much compressed he takes time and space for a moving letter from a woman to whom what he calls "harmonialism" is deeply important.

Ahlstrom saves one of his most important, one of his most general, insights for his last page. "The American experience," he writes, "does not explain itself." He who would look into the future, "whether as amateur or professional . . . will be a pioneer on the frontiers of post-modern civilization." He may be wrong in concluding that any such pioneer may find strength in the "idealism" of the country's

religious past; there are many other elements that Ahlstrom has himself in this book acutely illuminated. But no one writing or thinking hereafter about America's past will be able to ignore Ahlstrom's magisterial account of the religious element.

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RICHARD AUBREY MCLEMORE, editor. *A History of Mississippi*. In two volumes. Hattiesburg: University and College Press of Mississippi. 1973. Pp. xiii, 703; xi, 651. \$25.00 the set.

These encyclopedic volumes supersede all previous general histories of Mississippi. Forty-one authors, mostly natives of Mississippi or teachers at Mississippi colleges and universities, wrote the forty-three chapters. Most of the writers are professional historians, but the list of contributors includes a geographer, an anthropologist, an agronomist, and a sociologist, as well as a former governor and the present lieutenant-governor of the state. These highly factual essays begin with geography and prehistory and end with events of the 1970s. They offer most readers all the information they ever need to know about Mississippi.

These volumes suggest the healthy state of historical scholarship in Mississippi. Most of the essays rest upon extensive original research. In some chapters authors have summarized and updated their earlier writings. Thus John K. Bettersworth's judicious essay on "The Home Front, 1861-1865" is a condensation of his standard monograph on *Confederate Mississippi* (1943). Other contributors have expanded or synthesized their previous, more specialized studies. The elaborate documentation (together with the valuable bibliography compiled by Willie D. Halsell) indicates how much important work has been done in Mississippi history and is particularly useful as a guide to numerous unpublished theses and dissertations.

Despite the fact that these volumes were partially financed by the state of Mississippi most of the essays are remarkably objective. In only a few instances are Mississippians told that they "can take pride" in certain developments or informed that one of their politicians deserves to rank "as one of the country's greatest statesmen." If there is a certain amount of

pointing to Mississippi "firsts"—for example, to the fact that Mississippi established the first state-supported college for women—there is no attempt to conceal that Mississippi has all too often been last among the states, as in its rate of literacy and its per capita income.

In a refreshing break with tradition the editor, Richard Aubrey McLemore, who is also director of the Mississippi Department of Archives and History, has chosen 1890 as the dividing date to separate the two volumes. This arrangement gives implicit support to the growing conviction among historians that the Civil War-Reconstruction era marked much less of a break in Southern history than earlier scholars believed, and, of course, it gives much more space for extensive treatment of history of the past eighty years. Whether 1890 is more than a convenient dividing point in Mississippi history remains doubtful. Cumulatively the essays in these volumes show how astonishingly little the state changed up until about 1930 and suggest that the Great Depression and World War II brought about a striking transformation of Mississippi agriculture, industry, race relations, and, eventually, politics.

In a cooperative work of this scale there is inevitably overlapping, and even contradiction, among the essays, and McLemore has made no attempt to smooth these out. Indeed, except in the two final chapters, there are not even cross-reference footnotes to indicate that one contributor was aware that another had also dealt with his topic. Since nineteen of the chapters deal with broad subjects like geography, education, and labor, there is a considerable amount of repetition, and only the careful indexes bring together scattered discussions of the same topics.

McLemore's light-reined editing has also allowed enormous variation in the quality of the chapters. In general the first volume is sounder and more informative than the second. Especially valuable are Porter L. Fortune's analytical discussion of "The Formative Period," John E. Gonzales's sprightly chapter on "Flush Times, Depression, War, and Compromise," and William K. Scarborough's thoughtful account of Mississippi slavery. Except for Neil R. McMillen's admirable account of the "Development of Civil Rights, 1956-1970," the chapters on politics in the second volume are inferior in

quality. James P. Coleman's chapter on the Mississippi constitutional convention of 1890, which disfranchised blacks, shows how little a former governor and a present judge of the United States Circuit Court of Appeals understands either about his state or about history. William D. McCain's chapter on the Bilbo era is grossly partisan in its praise of an administration characterized by a contemporary as "a four-year orgy of graft, corruption, extravagance, political trickery, demagoguery and scandal in high places." Some of the topical chapters that make up the bulk of the second volume, such as Thomas D. Clark's superb essay on "Changes in Transportation," are highly instructive, but others dribble off into long lists of schools, colleges, judges, lawyers, hospitals, and the like.

Just because this impressive *History of Mississippi* is going to be, at least for a generation, a standard work it is necessary to mention its limitations. First, this is essentially a history of white Mississippians. Unlike some earlier histories of the state this is not a racist work. There are no slurs upon Negroes. On the contrary, the quality of black leadership in Mississippi during the Reconstruction period is described as "higher than that found in most southern states," and Mississippi Negroes are exonerated not merely of excessive political corruption but even of any special desire to hold public office. But references to the black half of the state's population are, throughout, few. As a result there is also little discussion of interaction between the races. William Lincoln Giles's judgment that in rural Mississippi there developed between blacks and whites "a mutual confidence, respect, and interdependence resulting in splendid race relations" is one of the few offhand remarks on that subject. Readers may be more impressed by the fact that "lynching" does not appear in the index of either volume.

Second, by focusing on the complex story of internal developments in Mississippi this history necessarily neglects the connections between the state and the rest of the nation. Occasionally one of the more thoughtful authors will offer a judgment like Glover Moore's conclusion that "in the 1850s Mississippians had more influence in Washington's ruling circles than ever before or since"; but there is no

systematic discussion of federal-state relations. Most United States senators and congressmen get little more than a listing in these pages, while every governor of whatever caliber rates a full-length discussion. This emphasis does not seriously distort the history of Mississippi in the nineteenth century, but since at least the 1930s the role of the federal government in agriculture, in industry, and in race relations has often been more important than the part played by local politicians.

The third problem is one posed by any state history: does a state really have a distinctive history? There is little in the useful chapters on economic, social, and cultural life in these volumes to indicate that Mississippi was much different from other states of the Deep South. Politically and administratively, of course, a state does have a history, but it is hard to find a central theme that will elevate state politics above antiquarianism. That task was made the more difficult in the present instance, where many writers worked quite independently of each other. Thus Fortune and Gonzales suggest that the early political history of the state revolves "around the challenge of the expanding areas of the state to the dominant position of the Natchez district"; William F. Winter sees politics of the more recent period as a perennial struggle of "Delta v. Hills"; and James G. Revels agrees with V. O. Key's analysis that the Negro is "the beginning and the end of Mississippi politics." But none of these somewhat contradictory suggestions is developed in a sufficiently systematic way to give unity to Mississippi's political history.

There is, of course, another kind of unity that the history of some states does have: the shared sentiments and beliefs of its inhabitants. There is, unfortunately, in McLemore's massive volumes too little discussion of what has made Mississippi such a distinctive and peculiar place, with a tenacious hold upon its natives even after they have long left the state. "Mississippi to me," wrote Tennessee Williams, who is one such expatriate, "is the beauty spot of creation, a dark wide spacious land that you can breathe in." Neither that spaciousness nor that darkness comes through in this work, which, after all, was not intended to be an interpretive history but a reference tool. An invaluable one McLemore's *History* is, too, and perhaps it will

assist some future W. J. Cash in writing "The Mind of Mississippi."

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ANDREW OLIVER, editor. *The Journal of Samuel Curwen, Loyalist*. In two volumes. Cambridge, Mass.: Harvard University Press, for the Essex Institute, 1972. Pp. xxxiv, 516; xi, 517-1083. \$30.00 the set.

Samuel Curwen has long been one of the best-known American loyalists, not because of his eminence within the loyalist community but rather because his fascinating journal was first published more than one hundred years ago, albeit in what now appears to have been an inaccurate and incomplete edition. As Andrew Oliver, the editor of this new version, points out in his introduction, George A. Ward, his predecessor (like too many other nineteenth-century editors), changed words and phrases, omitted some entries either in whole or in part, and combined others at random.

Curwen, a Salem merchant before the war, fled to England in 1775. Throughout nine years of exile he faithfully kept this journal, which is filled with incisive comments on his life in London and in various provincial towns like Exeter and Bristol. Unfortunately for those historians who have relied heavily upon him, however, Curwen was not truly representative of his fellows in at least one important respect, his attitude toward America. For example, in December 1776 he angrily wished for a rebel victory to convince "these conceited islanders" that America too had its "brave soldiers." Although his pro-American sentiment is often used as evidence of a change of heart common to other exiles Curwen's position was unique, and therefore to rely solely upon him is misleading.

Further, a comparison of Ward's version with Oliver's shows how the former's faulty editing has led historians astray for over a century. To cite only one instance: on February 1, 1776, Curwen attended a meeting of the New England Club, an exile organization, and recorded its membership. Ward, it turns out, added three members, altered Thomas Hutchinson's status in the club by omitting a significant phrase, and changed Curwen's "Mr. Waldo" to "Joseph Waldo," whereas the member in question was probably Francis Waldo, a customs

official from Massachusetts (Joseph was an American-born, pro-rebel Bristol merchant, so the difference is far from trivial).

Oliver's edition is, on the whole, accurate and meticulous. At times he neglects to identify Curwen's exile friends adequately: for example, Mr. and Mrs. Henry Barnes of Marlborough, Massachusetts, whom Curwen saw in Bristol, are never identified as Americans. At other times Oliver makes mistaken identifications, the most glaring of which (evidently through a transcription error) confuses the rebel Colonel Francis Marion with Colonel John Murray, a loyalist from Rutland, Massachusetts (p. 171). But such flaws are few and far between. Oliver has done a fine job, and this edition of Curwen's journal is a must for every serious student of the Revolution.

MARY BETH NORTON
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JACKSON TURNER MAIN. *Political Parties before the Constitution*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, 1973. Pp. xx, 481. \$15.95.

Professor Main's immensely thorough inquiry into preconstitutional American political parties exemplifies both the advantages and the risks of parliamentary biography—a method of historical investigation that we owe to Beard and Namier. The result is a systematic study of the votes in state legislatures wherever records exist in the Confederation period, amplified with biographical information going far beyond Beard's crude economic categories to include such matters as education, religion, and "world outlook." We are thus presented with vastly more, and vastly more reliable, information about the members of state legislatures than we have ever had before. Main likes to let his materials do much of the talking, while he assists them with numerous and useful tabulations. It cannot honestly be said that the author's style makes for readability; it often reads like a direct transcription from index card entries and is unfortunately likely to turn too many readers straight to his conclusions, which are not the safest guide to much of the state-by-state detail.

The broad results, however, are clear and impressive. Throughout most of the state legisla-

tures two groups emerge, defining themselves and opposing each other on the principal divisive issues with marked consistency. Main non-committally calls them "Cosmopolitans" and "Localists." These legislative "parties" identified themselves over the treatment of loyalists and their property, various forms of debtor relief, specific local issues such as that of the Bank of North America, and, above all, paper money. Main also finds a high level of continuity from the Cosmopolitans and Localists to the Federalists and Antifederalists and argues that these continuities will be found to survive into the party system of the 1790s.

Yet much will always depend on the questions that are put to the historical data and the categories that are perceived. These and similar issues certainly did mark divisions between great men and small, inhabitants of coastal and inland districts, town and country, and commercial and noncommercial farms in different states, but they did not always divide them along the same lines; there seems inadequate ground to presume that the issues themselves defined the categories either on a nationwide or even on a consistent local basis. On the contrary: Main himself finds that on paper money the Cosmopolitans of the South were thoroughly split (pp. 339-40), and in summarizing "The Issues" he remarks that "large landowners, on the average, divided just about in half and only rarely united" (p. 362). This constitutes a significant reservation to his conclusions precisely because the definitions themselves were reached through the analysis of voting records, to which should be added his own cautionary observation that "the present study tends to exaggerate the differences between blocs" (p. 359 n.11). Yet Main's conclusions have a relentlessly reductionist character, which persistently tends to emphasize the differences.

Hidden preconceptions may be partly responsible for occasional translations of the statistics into a sense they do not bear. Thus, on the restriction of slave importation during South Carolina's period of economic crisis, he writes: "The low-country majority for the restriction did not come from the planters, only half of whom agreed to deprive themselves of more Negroes, but from business and professional men: thus two-thirds of the latter ac-

cepted the amendment" (p. 285). Stated differently, the same figures show that this majority actually came from two-thirds of the businessmen plus no fewer than half of the planters; it is perhaps more remarkable that one-third of the businessmen opposed it. There is much consistency in the voting records that Main has found, but his language exaggerates it; eighty-one per cent becomes "almost always" (p. 290 and table 10.3), but eighty-one per cent can also be called four-fifths, and out of 200 is only 162!

Main is dismissive toward social issues, however important, if they "did not contribute to legislative bloc voting" (p. 354). Why? Because they did not contribute to legislative bloc voting. But the considerable number of neutrals disclosed by his researches seem to have been as consistent as the party faithfuls, while as many as a third of his "members" had inconsistent voting records. This finding is surely just as important for an understanding of politics as are the alignments that do appear on the issues that divide the representatives. "Inconsistency" exists only in relation to perceived consistencies. Party politics were widely deprecated, and many legislators plainly felt that neutrality was preferable to alignment, if not always attainable. These difficulties are involved with Main's basically unsatisfactory treatment of the concept of "party" in eighteenth-century politics, which leaves him groping darkly for an understanding of why the concept itself was so controversial.

He begins with the definition in Dr. Johnson's *Dictionary*; but he ignores Burke's tendentious but far more celebrated explanation, which virtually launched the modern idea of party (whatever Namier may have thought!). There need be no mystery about all this, since the idea of party came down from the terrible divisions of English politics in the seventeenth and early eighteenth centuries, with all their implications of disloyalty and even treason. But in this as unfortunately in other respects Main impairs his own position by his neglect of the conceptual and historical problems implicit in his inquiries and his disregard of the considerable literature that they have generated—most notably, Hofstadter's *Idea of a Party System* (1969)—with the result that he fumbles heavily with problems, concepts, and definitions that

have already been illuminated. His own criterion of party is merely mechanical: a two-thirds consistency in voting with one bloc constitutes "membership"; those legislators who voted this way are held to form a party. Whether they conceived themselves as doing so, how policies were concerted, what loyalties were invoked or formed—these questions, which are of critical importance to the idea of party, receive negligible attention.

Main's conclusions also raise unexamined problems of explanation. He treats virtually all strong statistical correlations as self-explanatory—and it should be said that most of his findings will satisfy the common-sense criteria of explanation, when carefully used. But he does not seem quite to appreciate that statistical correlations are not the same thing as explanations. He frankly states, perhaps overstates, the limitations of his methods with the warning that uncertainties and probable errors in the data give the statistics "an impression of accuracy that is quite false" (p. 43). This warning, however, does not touch the deeper problem of the connection between statistical correlation and historical causation. If these comments are critical, they are not meant to be ungrateful; they are made because of the obvious importance and usefulness of Professor Main's research.

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RICHARD WALSH, editor. *Journal and Correspondence of the Council of Maryland*. Volume 10, *Journal of the Council, 1789-1793*. (Archives of Maryland, volume 72.) Baltimore: Maryland Historical Society, 1972. Pp. xiii, 395.

This is the second volume in a series devoted to the journals and letters of the executive council of Maryland, 1784-1821. About eight more volumes—six of them devoted to the journals—should be needed to complete the set. The full cost of the venture will run close to \$200,000.

The journal consists almost exclusively of routine business: the issuing of commissions to local justices; the administration of bonds from civil officials; the consideration of applications for remission of fines and forfeited recognizances incurred at county courts; orders on the treas-

urers to pay accounts; or the routine execution of an act of assembly allowing indulgence on taxes. The lack of detail is discouraging. Although, for example, the editor claims that "the volume is filled with descriptions of the plight of many of the incriminated poor through their appeals for relief," there is next to nothing in the petitions for remissions of fines that would add more to the record in county court journals than the size of petitioners' families or the occasional names of victims. Nor do the very summary grants for tax indulgence "shed further light on Maryland's Confiscation Act." Although the journal would be essential to such supplementary work as the compilation of the names of officeholders in the period, it is hard to see how the bulk of the journal can be seen as a widely usable historical source in its own right. And I find it impossible to see how it illuminates the massive themes outlined in the editor's introduction (a "resurgence of the democratic ideals born out of the earlier phases of the Revolution"; "renewed agitation for manhood suffrage"; "the anomaly of slavery"; or "the struggle for mass education").

I have a prejudice in favor of public records. They have escaped the process of selection that sometimes makes private collections better guides to historiography than history. Their routine quality gives them an objectivity to which personal papers cannot pretend. And their massive comprehensiveness allows them to be used for the sort of sophisticated quantification that is so badly needed. But I think, too, that it is right to ask whether a record as limited in its research value as this really calls for the broad distribution of a letterpress edition. Were it the one surviving journal of a peculiar body, one among a few governmental sources for a period in which the remaining record was scant, or one weak spot in a series of much wider general interest there would be a good case for publication. As it stands, one wonders whether there is.

This is essentially a *document inédit*—a perfectly legitimate choice for an editor to make. But the few editorial revisions are cumbersome and inconsistent. To take one example, the introduction devotes a sentence to the spelling "goal" and adds that "other misspellings have been indicated by *sic*." Why it should be neces-

sary ever to add a "sic" to a spelling like "Relegion" I do not know. But to do so only erratically is downright irritating. More important, it is never legitimate for an editor, whatever his style, to look at a manuscript quite this uncritically. When, as on page 21, the same councillors are listed both in favor of a motion and opposed to it, a note correcting the text is imperative. And it requires little judgment to see that an abbreviation as obscure as "fund. tax" should be spelled out, especially when one as common as "tcs." has been awarded a bracketed "[tierces]." One can also demand an accurate and comprehensive index: one that includes references to the occupations (shoemaker, distiller) that are among the few useful details in the applications for remission of fines; that is not carelessly alphabetized (Black, Bladensburgh, Blacklock); that does not omit all the references to the armorer of Frederick Town on pages 182-83; that has adequate subject cross-references; and that does not confine the entry under "Accounts"—in a journal largely devoted to them—to two isolated pages. If the editors are not willing to engage in the elementary clerical pedantry that makes for a good edition, then that is one more reason for surrendering these valuable, but very restricted, records to the microfilm camera and letting them be.

R. NICHOLAS OLSBERG
*Colonial and State Records
of South Carolina*

L. MARX RENZULLI, JR. *Maryland: The Federalist Years*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1972. Pp. 354. \$15.00.

Politics were complex and colorful during the Federalist years in Maryland. Drawing from a wealth of correspondence, pamphlets, and newspapers Renzulli probes the motives and activities of political leaders for this period. His best chapters describe the intricate division over the ratification of the Constitution, with special attention to the speculation of leaders in script and confiscated property, and the evolution of a political party from the constitutional debate. The Federalist party, ostensibly led by an agrarian oligarchy, persisted from 1789 to the early 1820s despite internal factionalization—the result of regional and ideological stress—

and the growth of a popularly based opposition.

The wealth of contemporary political opinion, however, obscures Renzulli's critical vision. Since he construed party and leaders as synonymous he rarely looks beyond the actions of the elite. Terms like oligarchy or High Federalist, patterns of elite recruitment or constituent voting behavior, questions about decision making within the party, or dynamic concepts such as the meaning of an urban-rural conflict in this period are used imprecisely and remain unclarified. Indeed, he treats the Republican party entirely from a national and a Federalist perspective and explains little about its origins and activities on the state level. While he asserts that the Republicans were better organized than the Federalists, the infrastructure of both parties remains unexamined. Closer scrutiny would probably show that the Republican party was also led by an agrarian oligarchy.

As the definitive study of Maryland politics from 1787 to 1818 this book is informative but incomplete. Renzulli's handling of the material before 1800 is excellent, but such intensive interpretation does not endure for the remainder of the volume. Furthermore, no study of politics for this period is satisfactory without a detailed examination of the Republican party. Since Renzulli has explicated the behavior of the national-state political elite using traditional methods, now researchers should devote greater attention to the influence of regional competition within a state party system, to basic questions concerning elite recruitment and party decision making, to a more systematic analysis of voter support (as derived from pollbooks), and to the essential dimensions of primary concepts like oligarchy. Then, perhaps, we can better understand the impact of federal patronage on state-level politics or settle the anomaly of why areas of radical discontent during the Revolution became the backbone of the Federalist party.

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JOHN K. MAHON. *The War of 1812*. Gainesville: University of Florida Press. 1972. Pp. xii, 476. \$12.50.

American historians seem endlessly fascinated by the War of 1812, writing book after book

and article after article on some aspect of it. To them it is an important experience in the building of a nation, as well as a convenient model for testing historical interpretations. To English historians the war appears a minor episode. Occasionally they may recall, as one of them did, that "a few fir-built frigates, manned by a handful of bastards and outlaws," had injured the pride of the Mistress of the Seas.

John K. Mahon of the University of Florida is the latest scholar to turn his attention to the war. His study covers ground recently tilled by Reginald Horsman in *The War of 1812* (1969). But Horsman wrote a concise military history, whereas Mahon intended his work to be as nearly as possible definitive—a full-bodied account of encounters on land and water. Mahon believes that the past, particularly that which deals with warfare, can live only when presented in detail. So he explains in depth how and why Americans held their own and why the British failed to conquer. He analyzes tactics at the lowest level as well as high-level strategy. His theme, implied rather than stated explicitly, is that of blunder and futility; his story sketches a model of how not to use war as an instrument of national policy.

Although Mahon claims to have placed military operations "precisely within the politics and culture of the time" he has in fact written a straightforward military narrative that has in it little room for theory or broad analysis. He does suggest that American dissent over the war, and other developments outside the battle areas, affected diplomatic and military decisions. He does not, however, explore the causes of the war, mainly because it has been done often and hardly needs retelling.

Using both manuscript and secondary sources Mahon displays an impressive command of his specialized subject. He understands military affairs and is usually able to present technical data in terms meaningful to the nonspecialist. In general his prose is clear and uncomplicated. Some of his battle accounts, such as those relating the duel between the frigates *Constitution* and *Java* off the coast of Brazil, and the better-known naval encounter on Lake Erie, are especially well done. Also noteworthy are occasional bits of social history dealing with blacks and Indians. Even while fighting each

other, he reveals, Americans and Englishmen clung to their racist heritage. For example, despite the military advantages they might have gained, and the willingness of American blacks to switch to their side, British commanders discouraged the inciting of slave rebellions.

Regardless of Mahon's competence in military narrative the detailed descriptions of equipment used, of fortifications, and of battle maneuvers contribute little to large understanding. Yet the study has lasting value, for anyone interested in military affairs as well as for the scholar, as an authoritative reference work. Making good use of Admiralty Records and Canadian archives Mahon gives fuller coverage to the British side than have previous military historians. Anyone who desires a precise, connected account of a particular military operation will find it in this book. As much as any other feature this complete coverage makes the whole undertaking worthwhile. Rather than interpretation or new information, it is the book's main contribution to the literature of the subject.

ALEXANDER DECONDE
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JOHN M. McFAUL, *The Politics of Jacksonian Finance*. Ithaca: Cornell University Press, 1972. Pp. xv, 230. \$9.75.

The vagaries of Jacksonian finance have baffled and divided historians and economists from Jackson's day to our own. John McFaul has enriched the literature and has clarified some of the issues, but he has not arrived at a final answer. He takes issue with Bray Hammond's widely accepted thesis that the Jacksonians were not opposed to banks or business per se; they were only against the Bank of the United States and its relatively limited circle of beneficiaries. They were themselves enterprising businessmen who simply wanted to share in the distribution of capital. If this had been the case, McFaul argues, then the state banks on which the Jacksonians relied would have joined the opposition to the BUS. His evidence that the state banks tended rather to support that institution is weakened by the number of state legislatures attempting to tax branches of the bank out of existence. Once the government

deposits were shifted to the pet banks the money certainly was used to finance business enterprise, no different for being Democratic instead of Whig.

McFaul is on surer ground when he argues that the system of pet banks under Secretary Levi Woodbury was actually moving toward central banking, with the Treasury itself fulfilling the central role and the pets as branches. This trend was abruptly halted by the distribution of the surplus revenue, which left the system without operating capital. The final blow was the specie circular, which convinced the ordinary citizen that the government did not trust the banks. He hastened to get his money out while he could, and the crash of 1837 followed. Jacksonian finance, in a word, was probably just as confused as it seems. All shades of opinion were represented within the inner circle of the party, which made a constructive policy impossible for another decade.

CHARLES M. WILTSE
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SYDNEY NATHANS. *Daniel Webster and Jacksonian Democracy*. (The Johns Hopkins University Studies in Historical and Political Science, ninety-first series [1973], 1.) Baltimore: Johns Hopkins University Press. 1973. Pp. xii, 249. \$10.50.

ROBERT F. DALZELL, JR. *Daniel Webster and the Trial of American Nationalism, 1843-1852*. Boston: Houghton Mifflin Company. 1973. Pp. xv, 363. \$8.95.

Chronologically these two studies of Daniel Webster fit together rather neatly, the one concentrating on the years from 1828 to 1844 and the other on those from 1843 to 1852. But the two authors take somewhat different approaches.

Sydney Nathans, of Duke University, looks at Webster as an elitist politician who disliked parties and preferred "government by independent men" but had to respond to the rise of "voter-oriented partisan politics" in the Jackson period (p. 3). Nathans "analyzes his efforts to survive, comprehend, and manipulate the new politics" (p. 4). He finds that "Webster's adjustment to the issues and to the style of Jacksonian politics left him fit to cope with the voters by the end of the 1830s," but he was "less able to deal with the demands of his party comrades" (p. 6). Nathans concludes:

"Ironically, by 1844, Daniel Webster had once again become what he had been in 1828: merely a Massachusetts man" (p. 225).

Robert F. Dalzell, Jr., of Williams College, explains that he originally contemplated a "detailed analysis of the rhetoric of Daniel Webster's nationalism" but found that his rhetoric could not well be "treated separately from the rest of his career" (p. ix). Hence Dalzell has undertaken a restudy of Webster's politics along with his nationalism, choosing the period when nationalism "faced its severest challenge in America" (p. x). Dalzell sees Webster as a man "fighting simultaneously to save his political career and to defend something in which he believed most deeply" (p. xi). Believing that a "viable balance of sections, interests, and rights" was the "very essence of the American nation" Webster "devoted himself to articulating the principles of that balance" (p. xii). But in the end the "great vision" was "shattered on the paired reefs of sectionalism and democratic politics" (p. xv).

Despite their difference in approach the two books are similar in being essentially studies of Webster's role in presidential politics. The Nathans volume sticks somewhat more closely to this theme, tracing in considerable detail the man's opposition to Andrew Jackson in 1828 and 1832, his dream of a Webster-Jackson party after the nullification crisis, his unsuccessful Whig candidacy in 1836, and the frustration of his hopes for the nomination in 1840 and 1844. The Dalzell volume begins with an analysis of Webster's rhetoric in the second Bunker Hill address but, noting that by 1843 his "stock as a presidential candidate had fallen to zero" (p. 29), soon takes up the story of his renewed ambitions and continuing disappointments, with special attention to the "politics of compromise" in 1850 and to the final, bitter defeat in 1852.

Together the two books form an excellent and quite full account of Webster's political career during the quarter century they cover. Neither of the two alters the basic outlines of the familiar story, but both of them offer fresh insights and new, illuminating details. Both are based on extensive use of Webster and other manuscript collections as well as printed primary sources. The authors completed their

research before the preparation of *The Microfilm Edition of the Papers of Daniel Webster* (1971), a continuing project, under the general editorship of Charles M. Wiltse, that will lighten the work of Webster scholars in the future.

Certain views of each of the authors might be questioned. For example, Nathans emphasizes as distinctive of Webster his "dependence on patriotism as the way to secure the blessings of the traditional politics" (p. 7). But did not Jackson and the Jacksonians also appeal to patriotism in defense of the new politics? Again, Nathans implies that Webster favored a "Webster-Jackson party of patriots" because he wanted to "check the politics of conflict" (p. 73). But was Webster really more interested in that objective than in the more personal one of getting himself elected president? As Dalzell remarks in another connection, "party loyalty always mattered less to Webster than securing the object of his ambitions" (p. 55). Dalzell, for his part, makes the astonishing statement that "internal improvements could vitally concern only those producers, of whatever, who produced more than they could use themselves or sell locally—a relatively small group in America even as late as 1860" (p. 33). Were the wheat growers of the West and the cotton growers of the South producing only for local consumption? And did they constitute a "relatively small group"?

Both authors take Webster's political philosophy—his rhetorical nationalism, his concept of social harmony—pretty much as given. Neither author relates it effectively to the sectional economic interests that it served, though a close relationship would seem to be suggested by the fact that Webster had abandoned his earlier low tariff and states' rights ideas only when his constituency ceased to depend mainly on overseas commerce. Nor does either writer point out that Webster looked upon social harmony not as automatic (or as a product of individual virtue) but as contingent upon government policy and economic development, though he had made this clear in his earlier phase when he warned that protective tariffs, by fomenting manufactures, might give rise to a dangerous proletariat. Before 1828 he proposed *laissez faire* as the harmonizer, the means of avoiding class conflict; after 1828 he discovered the

proper means to be government aid to business enterprise.

To be sure, neither Nathans nor Dalzell pretends to be dealing with the prenationalist Webster, though each includes a summary of the earlier career. There is need for a new study of that earlier phase, a study conceived and executed with standards as high as those of these two books.

RICHARD N. CURRENT

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ROBERT H. RUBY and JOHN A. BROWN. *The Cayuse Indians: Imperial Tribesmen of Old Oregon*. Foreword by CLIFFORD M. DRURY. (The Civilization of the American Indian Series, volume 120.) Norman: University of Oklahoma Press. 1972. Pp. xix, 345. \$8.95.

The Cayuse Indians is a carefully documented yet most readable history written by a surgeon-historian team that has authored other works on the Indians of the Oregon Territory. In this book Ruby and Brown focus on the events surrounding the tragic occurrences of November 29, 1847, when a group of Cayuses entered the Methodist mission established eleven years earlier by Marcus Whitman and slew him, his wife, and twelve others.

During the late eighteenth century the Cayuse Indians, a plateau people who lived by hunting and by gathering wild vegetable foods, turned to horse raising and trading. Though a small tribe they acquired considerable prestige. Then the trickle of northwest explorers and traders gave way to a steady incursion of immigrant farmers who passed along the Oregon Trail through Cayuse territory seeking land and bringing dread new diseases. What had been sporadic clashes between Indians and newcomers took on a more serious note; Whitman himself felt something was bound to happen by the time of the massacre.

The events that followed the Whitman Massacre repeat a familiar pattern. The Cayuse lost the war that ensued and became divided among themselves as to whether to shelter or to give up the leaders of the attack against Whitman. Meanwhile, the massacre was used as official justification for a statement that, by their conduct, Cayuse lands were "forfeited by them, and justly subject to be occupied and held by

American citizens." The trial and execution of five leaders in the Whitman affair were followed by a decade of scattered fighting. The Cayuse unsuccessfully resisted the loss of their lands and were finally moved onto a reservation. Some later joined in the Nez Percé war of 1877, and some sought the revival of past independence through the Dreamer religious movement. In the end the Cayuse scattered and intermarried with other Indian groups. They no longer exist as a people; it is instead the sturdy horse they bred that bears the name by which they were known.

The authors tell the story straightforwardly, apparently committed to reconstructing the history of an Indian people without taking a partisan stand. Their position, however, seems strained to me. "We mention little about white responsibility for the massacre because it concerns the Cayuse Indians only indirectly," they write, yet they refer to the more militant Indians as "wily," "hostiles," "troublemakers," or "hotbloods," thereby implying the judgment they do not overtly make. It would have been better to engage in frank speculation about the role they see the various personalities in their account as perhaps playing in relation to various segments of Cayuse opinion. As it is, the Indians are categorized simply as either "friendly" or "hostile," and they do not sufficiently emerge as a diverse people actively attempting to deal with a difficult situation.

In a way, *The Cayuse Indians* is a guide for the more complete social and political history that remains to be written. Not that this is too serious a criticism, for it is a very good guide on a little-documented subject.

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C. THOMAS DIENES. *Law, Politics, and Birth Control*. Urbana: University of Illinois Press. 1972. Pp. vii, 374. \$15.00.

The spreading practice of birth control in the United States throughout the past century has challenged orthodox morality, traditional medical practices, and, not least of all, a host of federal and state anticontraceptive laws. C. Thomas Dienes, a student of the law, has therefore chosen birth control as a case study

of the relation of legal institutions to social change. His investigation makes it readily apparent that compared with the legislatures the courts have constituted a more accessible forum for those people, such as Margaret Sanger, who have promoted the practice of artificial contraception. Consequently Dienes emphasizes the case-law history of the birth control issue in what is assuredly the most exhaustive study to date of the legal aspects of this important subject.

The author, clearly sympathetic with the birth control movement, even more clearly endorses the principle of activist jurisprudence, "an explicit policy approach to constitutional decision-making" (p. 169). Great judges, in Dienes's view, are those who exploit opportunities to effect maximum changes in the law, consistent with minimum standards of continuity and order. Thus, for example, he praises the majority and concurring opinions in *Griswold v. Connecticut* (381 U.S. 479 [1965]), despite his recognition that "*Griswold* may cause some consternation to those who seek a craftsmanlike development of principled constitutional decision-making" (p. 177).

Unfortunately, birth control is not an encouraging example for advocates of such a theory. With very few exceptions the courts have accommodated only slowly and narrowly to the changes associated with the diffusion of contraceptive practice. Moreover, it could be argued that the law has not been particularly relevant to the modern history of birth control. Most of the "landmark" cases Dienes cites merely legitimized behavior that was already widespread and accepted. With regard to birth control, in other words, legal change has lagged far behind social practice—as distinguished, for example, from the area of civil rights where activist courts have been a significant force for social change.

This book is at its best when it critically discusses individual cases, especially *Griswold* and *Commonwealth v. Baird* (355 Mass. 746 [1969]). As history it is less valuable, often relying on dated and incomplete sources for the history of the birth control movement. In fact, though the author sets out to explore the connections between the law and society, he concentrates heavily on the law, especially on the relation between courts and legislatures,

while social history, in the end, gets rather short shrift. It should also be mentioned that, throughout the book, haste is hot, change nearly always comes in tides, people get "literally raked over the coals" (p. 288), and at least one development takes place, believe it or not, "back at the ranch" (p. 275). Prose such as this is as unscholarly as it is unjudicious.

DAVID M. KENNEDY
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DAVID F. MUSTO. *The American Disease: Origins of Narcotic Control*. New Haven: Yale University Press. 1973. Pp. xiii, 354. \$10.95.

There is gentle irony in David Musto's title for his study of narcotics in the United States, *The American Disease*, since Americans, at least for the past eighty years, have equated drug addiction with foreigners and suspect minority groups. Dr. Musto points out that the use of opium and its derivatives rose sharply in America during the second half of the nineteenth century and reached a peak in the 1890s, a decade when cocaine, too, had achieved great popularity as an all-purpose therapeutic and a favorite ingredient of medicine, soft drinks, and wines. The medical profession and proprietary drug makers share a good deal of responsibility for this rise of narcotic addiction, but, as Musto points out, the use of drugs grew far more rapidly in the United States than in any other Western country.

Many factors led to attempts to regulate the distribution of narcotics by 1900. The medical and pharmaceutical associations were seeking respectability, muckrakers were drawing attention to a wide range of social evils, and opium was associated with the Chinese minority and cocaine with Negro dope fiends. The failure of state and local laws led to federal action. The Harrison Act was finally passed in December 1914 because Southern opposition to strengthening the federal government was neutralized by an irrational fear of cocaine-crazed blacks.

During this period the question whether drug addiction was a disease or a moral problem was hotly debated, but both sides believed it was curable. By the end of World War I the moralists won out and addiction was ac-

cepted as a social evil that had to be stamped out. Addict maintenance programs were banned and the emphasis was placed upon rigorous punishment. This same climate of opinion prevailed until well after World War II when methadone programs revived the idea of maintenance. Public opinion today is once again swinging toward the view that addiction is a disease, but we are no closer to a cure than we were fifty years ago.

Musto's book is the first serious study of narcotic addiction in America, and it is a first-rate one. He shows that our policy toward narcotics has been an irrational, political one, conditioned largely by prejudices and social tensions. He clearly demonstrates that the amount of addiction is invariably exaggerated, that it is not a minority problem, that the relationship between crime and addiction has been largely one of society's making, and that addiction is a peculiar American problem. Our efforts to control the distribution of drugs on the international market simply reflect our delusion that addiction is un-American. I only hope this important book gains the widespread distribution it deserves.

JOHN DUFFY
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HOWARD B. SCHONBERGER. *Transportation to the Seaboard: The "Communication Revolution" and American Foreign Policy, 1860-1900*. (Contributions in American History, number 8.) Westport, Conn.: Greenwood Publishing Corporation. 1971. Pp. xix, 265. \$10.50.

Like a good many historical works, even of a specialized kind, this book presents very little that is absolutely new. Rather, it focuses upon a basic relationship that should have been emphasized long since but which somehow has not been. This is the underlying community of interest of American agrarians, American merchants, and American transportation enterprise in the development of effective routes of international trade. Likewise involved are questions of American foreign policy and even, in its broadest sense, the factor called "imperialism."

Professor Schonberger suggests that the traditional late nineteenth-century collisions between farmer and railroad baron have been

considerably exaggerated and that the story by no means should be restricted to a semimoral struggle between the deserving husbandman and the selfish Eastern capitalist. To be sure, there existed an inland jealousy of the older entrepôts—of New York, Boston, and Philadelphia—and of the already well developed trunk lines that led thereto. But the grain and cotton producer was by no means opposed to rail carriers as such, and the author takes pains to emphasize that a large part of the effort to reduce the mercantile New Yorkers to size involved the encouragement of alternative, but very similar, establishments at points like New Orleans, Galveston, and Seattle.

An important factor in the story is the astounding post-Civil War expansion of the American output of raw commodities for export, in particular grain, to such an extent as to create a chronic condition of glut in the markets of Europe. This naturally inspired a search for alternatives, notably in Latin America and the Far East, a search that produced demands for further railroad and port facilities and set off intensified discussions as to the possibility of a transisthmian canal and the advantages of a modernized navy. Made very clear is the fact that the American place in the structure of international commerce was, even so late as the turn of this century, essentially that of a producer of unfinished products. In fact, the world trade position of the United States was still of the "colonial" variety, even as the republic was assuming a clearly "imperial" posture.

This is a well-organized book: the author begins by stating his thesis; he then elaborates upon it and concludes with a comfortable summary. The scholarly apparatus is more than adequate. The errors noted are few and of the smallest consequence: Samoa is an archipelago, not an island; the Ocmulga River is usually rendered as "Ocmulgee"; and the Canadian Pacific Railway was not completed in 1886 (it was placed in full operation). This is, in short, a useful contribution to American economic history. If Professor Schonberger has perhaps pointed to the obvious, no one else seems to have had the perception to do so in quite the same way.

ROBERT C. BLACK III
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LOREN P. BETH. *The Development of the American Constitution, 1877-1917*. (The New American Nation Series.) New York: Harper and Row. 1971. Pp. xxvi, 280. \$7.95.

There has long been a need for a fresh overview of late nineteenth- and early twentieth-century American constitutional history. The political history of the period has been subject to stimulating reinterpretation; but the character of governmental and legal thought and institutions has not yet undergone a comparable re-examination.

So one might reasonably expect to look for fresh and interesting analysis in the New American Nation series volume on the constitutional history of the 1877-1917 period. But it cannot be said that Professor Beth's work fulfills that expectation. The problem, I think, lies in his apparent inability to decide whether he is writing a work of history or of political science. In subject matter it is clearly the former; in approach it is more so the latter; in execution it falls somewhere in between.

Professor Beth's first four chapters—half of the text—examine the major political and governmental institutions of the time: the presidency, Congress, the bureaucracy, the political parties, the elective system, and the courts. His analytical mode is that of the first generation of political scientists—Woodrow Wilson, W. W. Willoughby, Frank J. Goodnow, Henry Jones Ford—who defined the nation's Constitution as the sum of its public institutions. This might have been a useful approach if it had reflected the insights of more recent political scientists such as David Truman and David Easton. But there is little of this; and space limitations prevent the author from offering more than a hurried survey of these topics and concluding that governmental power on every level was becoming more active and more concentrated.

The remainder of the book is given over to the activities of the nation's courts. Professor Beth retells the familiar story of the Supreme Court's use of substantive due process to limit state business regulation, and its failure to use that concept to protect the civil rights of Negroes. Again, his conclusions are unexceptionable—and unexceptional: that the Court was not wholeheartedly pro- or anti-business

and that its decisions had the effect of increasing its own determinative power.

The most original portion of the study examines the work of the state courts (chapter 8). Professor Beth usefully reviews a wide range of cases dealing with economic regulation and individual rights. But that he can draw from this survey only an "impression of overwhelming confusion" (p. 229) perhaps speaks more to the limitations of his approach than to the realities of the time.

Professor Beth rarely attempts to describe the evolution of judicial doctrine from 1877 to 1917 as he does with government and politics during that time. The progressive impulse of the early twentieth century had an evident impact on the presidency and on domestic and foreign policy; was there no concomitant development in the courts? There is no way to tell; the author does not ask this question, let alone try to answer it. He tells us that great constitutional change occurred between 1877 and 1917. But he has not really shown us the character of that change so far as the courts are concerned. To conclude that by the latter date "American constitutionalism remained invincibly middle class" (p. 252) begs the question whether the character of middle-class perceptions of law and government in 1917 were what they had been fifty years before. It is hard to believe that this was the case.

MORTON KELLER
Brandeis University

ALEXANDER B. ADAMS. *Geronimo: A Biography*. New York: G. P. Putnam's Sons. 1971. Pp. 381. \$8.95.

Geronimo, purportedly a biography of the Apache war leader, is more so a history of the Apache nation and its relations with the United States and Mexican governments during the nineteenth century, and of the bloody contest between Apaches and Anglo-Americans for control of southern New Mexico and Arizona. The author includes sketches of Geronimo's antecedents—Mangas Coloradas, Cochise, Nana, and Eskimizin—Apache patriots who defended their homeland and throttled the American advance into the Southwest's heartland during most of the nineteenth century. Some attention

is given to the Apaches' simple life-style, their martial tradition, and their propensity for raiding the Spanish-American settlements as a source of captives, plunder, and glory. *Geronimo* is a literary tragedy, a chronicle of admirable resistance to American intrusion, an epic of ethnic destruction. The seriousness of the Apache threat was confirmed by the national government maintaining an army of 5,000 troops and 500 Indian scouts in an extended Southwestern campaign to search out and destroy Geronimo and his band of nineteen warriors. Upon his capitulation in 1886, Geronimo and his warriors with their families were sent to Fort Pickens, Florida, as prisoners of war. The captives were relocated to Mount Vernon Barracks, Alabama, in 1887 and five years later were moved to Fort Sill, Indian Territory.

The life story of the Apache leader is based largely on secondary sources and contains little that is new except the author's field work, a comprehensive study of geography of the Apache homeland. Thus on the subject of the Apache milieu he can write with certainty and authority. The descriptive passages depicting Apache haunts in northern Mexico, southern Arizona, and New Mexico rate as the most valuable part of the author's effort. Likewise, he has compiled an extensive list of printed sources on Geronimo not hitherto available. *Geronimo's* faults are extensive, the principal one being viewpoint. Occasionally it is presented from Geronimo's stance, but more often in the broader Apache nation context, shifting from this to the United States Army viewpoint with such frequency that the reader's continuing question must be "Where is Geronimo?" This maze of shifting viewpoint obscures the principal figure and occasionally loses him completely. *Geronimo* lacks essential continuity in content and, all too often, the chronology is vague. The publisher touts *Geronimo* as "unequaled in depth and scope," adding the unwarranted claim that "here is the definitive biography of Geronimo," which clearly it is not.

ARRELL M. GIBSON
University of Oklahoma

PETER KOLCHIN. *First Freedom: The Responses of Alabama's Blacks to Emancipation and*

Reconstruction. (Contributions in American History, number 20.) Westport, Conn.: Greenwood Press. 1972. Pp. xxi, 215. \$10.00.

This instructive case study of Alabama blacks during the first half decade following the Civil War centers not on the actions of whites toward the freedmen but on the ways in which the Negroes themselves responded to emancipation. Consisting of a series of chapters on migration, the black family, education, churches, class structure, and politics the book presents data demonstrating that the freedmen displayed a vigorous independence and played a vital role in shaping the character of the Reconstruction experience and of the postwar black community. Thus, for example, sharecropping developed largely because the former slaves preferred it to a wage system, and despite the demoralizing effects of slavery black family life assumed a stable patriarchal character. The best chapter is the one on politics, where the author demonstrates in a striking manner that blacks operated as a largely autonomous bloc and were anything but subservient to the white scalawags and carpetbaggers in the Republican party.

The author's conclusions buttress those of scholars like A. A. Taylor, Vernon Wharton, and Joel Williamson in regard to education and the church. His discussion of agricultural labor supplements that of Wharton, Williamson, Martin Abbott, and William McFeely, though his interpretation of the Freedmen's Bureau in this connection is more favorable than other recent research. And overall the author goes far along the lines of inquiry suggested in the discussion of the black experience during Reconstruction in Harold M. Hyman, ed., *New Frontiers of the American Reconstruction* (1966).

Although neither the approach nor the conclusions are as original as Kolchin's introduction would lead the reader to believe; although the section on class structure suffers from inadequate conceptualization because the author in effect equates economic class with social class; and although the author often writes with unwarranted certitude on the basis of limited or ambiguous evidence, *First Freedom* is a welcome addition to the growing number of studies approaching Reconstruction in the

South from the perspective of the black experience.

AUGUST MEIER

Kent State University

I. A. NEWBY. *Black Carolinians: A History of Blacks in South Carolina from 1895 to 1968.* (Tricentennial Studies, number 6.) Columbia: University of South Carolina Press, for the South Carolina Tricentennial Commission. 1973. Pp. xiii, 388. \$9.95.

Malcolm X used to say that black Americans did not land on Plymouth Rock, rather Plymouth Rock landed on them. Professor Newby might have borrowed this image to introduce the central theme of his book—namely, that white supremacy crushed the life out of black Carolinians in the dismal period between the disfranchisement campaign of 1895 and the Orangeburg Massacre of 1968. "White Reconstruction" betrayed black Reconstruction as blacks in twentieth-century South Carolina succumbed to "white codes" in a "white state"—a "closed society" governed by a "gross racism." Lynching, peonage, disease, illiteracy, poverty, and powerlessness are the familiar code words that spell out a history of oppression and squalor.

The author portrays a half-century of dishonor, but he does not give us a full picture of black Carolina. He rationalizes that he could not produce a study of the black community, because, except as a "geographic expression," there was no such community. Indeed, he comes close to saying that there existed a black anti-community, bereft of leaders, plagued by parasitic institutions, and inhabited by stunted individuals whom white Carolina had "programmed" as "good darkeys"—New South Sambos.

In short, the offspring of white supremacy is black pathology. There is truth here, of course, but it is partial truth. Newby looks on black religion as an opiate that "engaged Satan in the fiercest kind of combat" but found "the white man too menacing to challenge." A more subtle analysis might have perceived that within black religion Satan was the white man. Newby describes the 1903 Columbia streetcar boycott as a failure, which it was, but he makes no effort to analyze the move-

ment and its legacy for twentieth-century black protest. Similarly we learn too little about the place of Booker T. Washington in the history of black Carolina; the same can be said for the NAACP; Marcus Garvey is absent even from the index.

Admittedly the Garvey movement was not to Charleston what it was to Harlem, but Newby's failure to mention the greatest grassroots movement in Afro-American history is symptomatic of his larger failure to penetrate the substrata of black Carolina. His advice that black Carolinians, like white Populists, should have "raised less corn and more hell" seems a gratuitous non sequitur following his presentation of the masses as the broken victims of racism. He depends entirely on published materials, mostly the public documents and white newspapers of South Carolina. Collections like the Booker T. Washington and NAACP Papers, as well as oral and folk sources, would have enriched his study. Even a section of good photographs would have warmed the clinical tone of the book.

These misgivings ought not to detract from the considerable virtues of Newby's work. In venturing beyond the patented years, 1865-1900, he has launched a second generation of state studies on Southern Negroes. There is a Myrdalian completeness to his statistical profile of black Carolinians, and his unsparing account of racial injustice is vital to these Tricentennial Studies. One can appreciate the book as it is while wishing for the book that it might have been. Professor Newby has shown us the black Carolina that suffered, which is important; but not the black Carolina that endured, which is more important.

WALTER B. WEARE
*University of Wisconsin—
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RICHARD D. CHALLENGER. *Admirals, Generals, and American Foreign Policy, 1898-1914*. Princeton: Princeton University Press. 1973. Pp. viii, 433. \$16.00.

This long-awaited volume by a pioneer in the field of civil-military relations analyzes the ways in which the officer corps viewed international affairs and the manner in which the diplomats utilized armed force to bolster foreign

policy. It makes no pretense of being a full-fledged study of national security procedures, such as one might attempt for the period after 1945. It concentrates on the reciprocal relationship between the presidents and secretaries of state on the one hand and the uniformed services on the other. It says little about Congress and the committees that deal with the army and navy. Challenger finds that most officers sought not a larger share in policy-making but rather clearer guidelines within which to plan for war and peace. In an era of imperialism, military and civilian leaders shared many assumptions about the world in which they lived, the power alignments in Europe and Asia, the use of force, and the need for stability in the so-called backward areas. Concern for the nation's security often led to exaggerated fears among generals and admirals, but a similar lack of realism could be found in the White House and State Department as well as in legations and consulates abroad. The author concludes that interservice collaboration before 1914 far exceeded that between the civilian and the military.

Arguing that personalities, not institutions, shaped policy, Challenger tells his story by presidential administrations. Of the four long chapters that comprise three-fourths of the text two are devoted to McKinley and Roosevelt and one each to Taft and Wilson. Besides a full introduction and conclusion there is an initial chapter that explores the world view of the officer corps, the extent of interservice cooperation, communication channels between civilian and military, and contemporary practices in Europe. The volume rests largely on unpublished records in the State, War, and Navy Departments and on a wide array of personal manuscripts. Particularly rewarding are the papers of the Joint Army-Navy Board and the Navy's General Board, the subject and area files in the Naval Records and Library Collection, and the reports reaching the secretary of state from commanders maintaining a naval watch in East Asia and the Caribbean. The omission of these last reports in the *Foreign Relations* volumes has led historians to overlook one military contribution to foreign policy. Thus, Rear Admiral Joseph B. Murdock, reporting from Shanghai in 1911-12, offered a different—and often more realistic—

view of the Chinese revolution than did Minister William J. Calhoun in Peking. Similarly, Secretary of State Philander C. Knox came to pay more attention to the dispatches of Rear Admiral William W. Kimball from his squadron in Nicaraguan waters in 1909-10 than to those of the often hysterical or self-serving departmental representatives in that strife-torn country.

To diplomatic historians, who usually ignore the records of the armed forces, this volume should add to their knowledge of many events—the Boxer revolt, the Venezuelan blockade, the uprising in Panama, the turmoil in Central America, the tensions with Japan, the downfall of the Manchus, and the intervention in Mexico. To those who are not familiar with recent work in military history Challener provides the best single source of information on such continuing problems as the defense of the insular outposts, the quest for naval bases, the location of the battle fleet, the evolution of war planning, and the use of gunboat diplomacy. Because the book has been long in preparation some of its findings have been anticipated by William R. Braisted, Arthur S. Link, Charles E. Neu, and others, while its main outlines are already known to the small but growing band of specialists in national security policies. Nevertheless, in the questions it poses and the sources it taps this work can serve as a starting point for similar but much more needed studies of the periods 1921 to 1939 and—it is hoped, before too long—1939 to 1950.

RICHARD W. LEOPOLD
Northwestern University

C. ROLAND MARCHAND. *The American Peace Movement and Social Reform, 1898-1918*. [Princeton:] Princeton University Press. 1972. Pp. xix, 441. \$16.00.

A proper understanding of this book requires a full appreciation of the relationship between peace and domestic reform movements suggested in the title. Anyone familiar with the overlapping leadership and similar tactics of the peace and civil rights movements of the 1960s is aware of the potential rewards from careful study of such relationships; Marchand has utilized this approach on a broad scale

for the so-called Progressive era. In separate chapters he describes the involvement of international lawyers, businessmen, "scientific" educators, women suffragists, social workers, labor leaders, and churchmen in the American peace movement. The author skillfully analyzes a hodgepodge of more than one hundred individuals and various groups. The result is a study of extraordinarily diverse approaches to foreign policy questions. Marchand emphasizes that the foreign policy views of the peace spokesmen derived from their professional and occupational goals as well as from their prior notions of domestic society and social reform. He explores this dimension so exhaustively and persuasively that it should never have to be done again.

Marchand's approach is interesting and plausible, but it also creates methodological and interpretive problems. His focus on the relationship between peace and various domestic reform concerns inevitably highlights minor figures in the peace movement who were deeply involved in other causes while slighting many of its central actors who almost exclusively promoted international reform. We learn much more about Carrie Chapman Catt, for instance, whose priority was suffrage than about a dedicated pacifist like Emily Greene Balch. He also exaggerates the monolithic, establishment characteristics of the peace movement for the 1898-1914 years. Some historians of anti-imperialism might object to his emphasis on the similar viewpoints between anti-imperialist peace advocates and those imperialists who joined the peace movement after about 1900. His interpretation is not surprising, however, because he perceives little difference between the anti-imperialists' promotion of the expansion of American ideals abroad through the use of righteous example and the imperialists' willingness to extend them by force. A convincing evaluation of the peace movement at the end of the century will require more research on what the peace leaders actually said and did during the debates over war and imperialism. Marchand also neglects divisions over military preparedness before 1914 and probably overstates the similarities between the World Peace Foundation and the Carnegie Endowment for International Peace.

One can readily agree with Marchand's con-

clusion that the peace movement was a "second-class" reform if the standard of judgment is absolute pacifism. But it is an open question whether the quest of the political internationalists for a League of Nations constituted a second-rate movement. Marchand's preoccupation with social relationships unfortunately inhibits any extensive discussion of the political impact of that or any other "peace" campaign on foreign policy issues. Finally, it may be that the weakness of the peace cause stemmed less from competing domestic loyalties than from more fundamental forces in American society—in particular, the self-confident nationalism of the era and the absence of any prolonged military crisis that might have prompted widespread public revulsion against the sanctity of the nation-state.

Many of these queries are admittedly debatable, and scattered remarks in the book indicate that Marchand is at least aware of some of them. It is the great merit of this book that it provokes so many questions on the study of peace movements. Fully researched, boldly conceived, lucidly written, and forcefully argued, Marchand's volume is an important contribution to the growing literature on American peace movements.

DAVID S. PATTERSON
Rice University

WALTER JOHNSON, editor. CAROL EVANS, assistant editor. *The Papers of Adlai E. Stevenson*. Volume 1, *Beginnings of Education, 1900-1941*; volume 2, *Washington to Springfield, 1941-1948*. Boston: Little, Brown and Company. 1972; 1973. Pp. xxi, 586; xiii, 620. \$15.00 each.

The career of Adlai E. Stevenson well exemplifies what persistent vitality the principle of hereditary, one might almost say territorial, aristocracy retains in American politics. The son of a secretary of state for Illinois, the grandson of a vice-president of the United States, the great-grandson of Jesse Fell, Adlai Stevenson could trace—and did trace, with a good deal of modest oratorical reiteration—his family's prominence in the public life of Illinois over virtually the whole of the state's political history. Nothing could be less indicative of such an inheritance than Adlai's accident of birth—in Los Angeles, of all places. But this was swiftly corrected by his return,

at the age of six, to the family heartland at Bloomington, where the *Daily Pantagraph* was long established as a Stevensonian journalistic fief. His Middle Western upbringing was a prelude to Eastern schooling: at Choate and Princeton, with both of which there were family connections, then to the Harvard Law School, and so back, nearly full circle, to the La Salle law firm in Chicago in 1927.

It is not the least virtue of the first volume of this impressively comprehensive collection of *Papers* that it provides full material for the reconstruction of this background to Stevenson's later career. Its importance for him, in endowing him with an easy assurance of springing from the central stock of his region's, indeed his country's, political and moral leadership, becomes abundantly evident. The letters of childhood and youth, as graphically as snapshots in a family album, set Stevenson as the favored (but not spoiled) son of a devoted family, which in turn occupies an assured (but also earned) place in the life of a basically stable community. Their very ordinariness, their straightforward, predictable reactions to the challenges and pleasures that American life offered to a young man of average attainments in above average circumstances make—not indeed for very lively reading—but for a proper comprehension of the roots of Stevenson's liberalism. For this liberalism, from which a decade of anxious and harried citizens, from the urban intelligentsia to the rural poor, drew sustenance and courage, was not nurtured in youthful rebellion or frustration. It was a natural expression of an inherited tradition that Stevenson sought not to flout but to fulfill. When his turn came he, too, married into an old Chicago family, his children went East for their education (and, when in London, to Harrow), and he accepted rather than solicited the burdens and opportunities of public life.

The familiarity of this Whig tradition and setting (one is reminded repeatedly of the correspondence and career of the youthful Franklin Roosevelt) necessarily robs the greater part of the first volume of these *Papers* of any element of novelty or surprise. Indeed as one item of misspelled *juvenilia* follows another it is tempting at moments to inquire whether they fully earn their keep in these admirably

printed, spacious pages. The editors must often have had to wrestle with such a question themselves. On balance the reader, even as he exercises his basic human right to skip, must applaud their decision to go for completeness, especially since it is not apparently being bought at the price of excision elsewhere. (The decision to print only a selection of the letters from Stevenson to his first love, Miss Birge, was presumably dictated by other considerations.) One thing that does emerge very clearly from these letters is the close affection between son and mother and its unmistakable tinge of possessiveness. (Mrs. Stevenson's coming to live in Princeton during his second year is strongly reminiscent of Sara Delano Roosevelt's similar infliction on her poor son at Harvard in the 1900s.) The correspondence also demonstrates the peculiarly intimate rapport between Adlai and his sister; it is clear that no adequate life of Adlai can be undertaken without an understanding of the remarkable personality of Mrs. Ives.

In 1928 Adlai was writing to Miss Birge, "I've become one of the standardized earnest young men whom I used to despise so wholesomely" (vol. 1, p. 201). It is tempting to credit the challenge of the New Deal for breaking him out of his corporation lawyer's shell. Unfortunately the letters surviving from his year at the Agricultural Adjustment Administration are too few to substantiate this theory, though they do make it evident that his New Deal, well represented by the George Peck faction at the AAA, was far from a revolutionary experience. By July 1934 he was ready to "go back to Chicago and finish what I started—make my place etc" (vol. 1, p. 258). Nor does the ensuing quinquennium as a "Chicago civic leader" give much indication of what lay ahead; the reader will quickly weary of the succession of charming, light-weight, carefully uncontroversial chairman's introductions at meetings of the Chicago Council on Foreign Relations.

It is in fact the challenge from abroad, not at home, that seems to have made Stevenson "come out" and grow up. The events of 1940 and 1941 and his work with the William A. White Committee elicit a new kind of commitment and an altogether sharper response. When volume 2 opens, six months before Pearl

Harbor, he is ripe for the unquestionably formative experience of being assistant to Frank Knox, secretary of the navy. There is excellent reading in the papers of these years, ranging from some unusually penetrating strategical and logistical assessments to the lively diaries that Stevenson kept while accompanying the secretary on service missions and subsequently on his Foreign Economic Administration mission to Italy.

The failure (on which the *Papers* are very thin) on Knox's death to secure control of the *Chicago Daily News*, or even to protect it from the emasculation that quickly succeeded its sale, meant that the way into public life via journalism was substantially closed. Yet it was still true, as Stevenson had written in 1941, that he never "fancied" himself "as a combatant politico" (vol. 2, p. 26). In these circumstances it was fortunate that Washington never ceased to coax him back to service in the executive branch, and that despite the intermittent obligations of his law practice he never failed to respond.

So began the period of Stevenson's service with the State Department and the infant United Nations, admirably documented here with letters, diary extracts, speeches, and official statements, which cumulatively build up a picture of these formative years that no historian of the UN can ignore. Only the San Francisco Conference itself is passed over in virtual silence: it was an unhappy meeting for any American P.R.O. But on a host of other topics—Chicago's bid for the UN site, the Russian role in the Preparatory Committee—the *Papers* are highly illuminating.

Despite it all, Stevenson's forty-seventh birthday found him "restless" and "dissatisfied." "How can I reconcile life in Chicago as lawyer with consuming interest in foreign affairs?" (vol. 2, p. 365). The answer came, oddly and in the *Papers* not wholly comprehensibly, via the governorship of Illinois. But it is clear that something ignited him "as a combatant politico." One senses here a new joy in combat and a new release.

The intrinsic interest of these *Papers* has left all too little space for comment on their editing. But this is in fact the highest tribute. So careful and unobtrusively helpful is the editors' discharge of their obligations that the

reader's attention is kept focused where the editors would clearly wish—on the *Papers* themselves. These two handsome volumes are a monument not only to Adlai Stevenson but also to Professor Johnson and Miss Evans and constitute a triumphant inception of an admirably planned and executed undertaking.

H. C. NICHOLAS
New College,
Oxford

RAY ALLEN BILLINGTON. *Frederick Jackson Turner: Historian, Scholar, Teacher*. New York: Oxford University Press. 1973. Pp. x. 599. \$17.50.

As each age, in the words of Frederick Jackson Turner, "writes the history of the past anew with reference to the conditions uppermost in its own time," so what others write of Turner describes them and their times as well as him. Since virtually all American historians—even Walter Prescott Webb, who said that he had not read Turner when he developed his own interpretation of American history and published his first books—are in some degree Turner's intellectual heirs, what they see in him has varied widely. Thus we have had views of Turner as a romantic and as an analytical historian, as monist and as pluralist, as apostle of democracy and of imperialism. Ray Billington says (p. vi) that he chose to write Turner's biography as the biography of a college professor and that he derived this book from a draft of two thousand pages that is available for fuller reference at the Huntington Library. In the ten years since Billington left Northwestern University to go to the Huntington as senior research associate, he has also produced three other books about Turner, *America's Frontier Heritage* (1966), an appraisal of Turner's interpretation of the frontier and of the views of his critics; *"Dear Lady": The Letters of Frederick Jackson Turner and Alice Forbes Perkins Hooper, 1910-1932* (1970); and *The Genesis of the Frontier Thesis: A Study in Historical Creativity* (1971). The biography and its predecessors are not so different from other books as they might have been if other writers on Turner (such as Richard Hofstadter, in *The Progressive Historians* [1968]; Wilbur R. Jacobs, in *The Historical World of Frederick Jackson Turner*

[1968] and other works; and Howard R. Lamar, in *Pastmasters* [1969]) had not drawn on Billington's work, especially on his articles. But readers who have heard Billington read papers on Turner will recognize distinctive interpretations, emphases, and style. While Billington apparently has used all published accounts of Turner and many collections of manuscripts (the author describes forty-three of "the most important" apart from four collections of Turner's papers, which must constitute a record in the genre of biographies of college professors), the book, like its predecessors, is much a product of this time, of the vast resources of the Huntington Library, and, above all, of a historian committed to the significance of the frontier in American history as Turner described it. It will not take the place of the many published recollections of Turner by his students or of the book that Fulmer Mood or another student might have written, analogous perhaps to Ralph Barton Perry's *Thought and Character of William James* (1948), but it is difficult to imagine anyone else now retracing life and labors in such detail and with the enthusiasm for the task that makes this long book engrossing reading.

Thirty-five years ago Charles Beard, writing on *The Frontier in American History* in a series of essays that the *New Republic* (92 [1939]: 359-62) published on books that changed their readers' minds, disagreed with Turner's interpretation of the role of the frontier but paid tribute to him as prophet of new directions and dimensions in historical research and as a selfless and inspiring teacher. Beard wrote that "personally Turner was one of the most modest and diffident scholars ever produced in America." Billington does not quite try to turn that estimate around but goes to considerable pains to justify Turner's theories and interpretations by standards of later generations: "his presentations [in diplomatic history] were so correct, that they were immediately absorbed into textbooks as part of the acknowledged pattern of history" (p. 170); he "came to a surprisingly correct conclusion concerning the frontier process" (p. 456). The one area of scholarship where Billington seems to me to be overly severe is historical statistics (pp. 468-69); here he draws on some of Turner's own criticisms of the maps that he used in

writing on the United States of 1830-50. Criticism is natural enough in an age when social scientists have excelled—in part, perhaps, because of pushes that Turner gave to them—in the mechanics of measurement, in what the late Pitirim Sorokin called quantophrenia. But do not present-day cliometricians still look back with respect to Turner and his students, from Orin Grant Libby ("A Plea for the Study of Votes in Congress," *AHA Annual Report* [1897]) to Merle Curti? Turner's enthusiasm for making correlations by precincts and wards in the Wisconsin Domesday Book as Joseph Schafer proposed it (1920) seems statistically more sound than the compilation of demographic and electoral data by counties at the Inter-University Consortium for Political Research.

At the same time, Billington, whose years of early retirement from teaching have been so different from Turner's, cannot forgive Turner, as Beard did, for not writing more books, and Turner's dispersion of his efforts is almost the main theme of the book. Even in his declining years at San Marino, "it would have been far better if he had had the will-power to refuse . . . invitations to write and speak and consult" (p. 406). Beard's view of Turner's output ("That was in truth enough for one man to do in a lifetime.") was close to that of Carl Becker, who said that "history, as Turner [conceived] it, [was] not well adapted to quantity production" (Becker in Howard W. Odum, ed., *American Masters of Social Science* [1927]: 312, 314, 317). Further, Billington refutes Beard's picture of modesty, telling at length of Turner taking private pleasure in laudatory book reviews while evading his commitments. Perhaps such problems are insoluble, especially if, as Billington suggests, Turner's uncompleted last books lacked the originality of his early essays and if he lost touch with the social sciences, from which he had drawn hypotheses and method in his youth. Turner may have been, far from Tolstoy's historian as a deaf man, like a man with very good hearing who in his best years spent much time listening to questions that many others were asking; the answers that he gave, short as they were, have had more influence than those of colleagues who efficiently insulated themselves from interruption.

Turner's twenty-nine years as a student and teacher at Wisconsin account for most of the narrative part of the book (pp. 18-57 and pp. 82-307, as against pp. 309-415 for his twenty-two years at Harvard and after), as they do of most memoirs of him. We learn much of Turner as citizen and leader of the academic community in president-making, in departmental affairs, in extension teaching. When he left Wisconsin, it was to help the university by showing the regents the consequences of their attacks on it. "Turner would have sold his soul to stay in the Madison that he loved" (p. 302); he was not dedicated to Harvard and was not at home at Cambridge (p. 385). Billington apparently is not impressed by Hofstadter's suggestion that Turner went to Harvard hoping that a change of scene would help him to write more, and that Cambridge offered a livelier atmosphere, a new intellectual frontier. (According to William B. Hesseltine, Mrs. Turner told him that her husband never regretted the move.)

Telling much of Turner's personal affairs, Billington does not give so clear a picture of either university or of Turner's associates, including students whose ideas, like Carl Becker's, are not easily encapsulated. Broad strokes sometimes oversimplify background, and peripheral details falter. Wisconsin in the 1890s was "a backwater college . . . overshadowed by the great eastern universities" (p. 142), though when Woodrow Wilson, described as a member of the department of history at Princeton, tried to persuade Turner to leave it, he had to apologize for Princeton's library (p. 152). That President Van Hise encouraged the faculty at Wisconsin to favor humanistic above practical research (p. 292) seems an overstatement of his defense of the humanities and social studies. The effect of exemplary research is slightly marred by typographical errors and slips in names and identification of minor characters. But perhaps Turner's students and Turner's place with them in the changing world of scholarship call for still another book. As a whole, this one is a major event in the history of higher education and of American historiography.

EARL POMEROY
University of Oregon

ROBERT D. CUFF. *The War Industries Board: Business-Government Relations during World War I*. Baltimore: Johns Hopkins University Press. 1973. Pp. x, 304. \$13.50.

Although industrial mobilization was one of the most significant aspects of America's World War I experience historians largely neglected it for over half a century. This impressive monograph deals with an important aspect of that experience by examining the role of the War Industries Board (WIB) in coordinating the war efforts of government and industry.

Cuff's well-researched study does much to illuminate the development, operation, and dissolution of the WIB. He also provides new information about its predecessors, the Council of National Defense and the Munitions Standards Board, as well as the war service committees composed of businessmen who represented individual industries. Most of the book is devoted to an intensive examination of the WIB's operation, especially the formulation of industrial policies, priorities, and price fixing. Throughout his discussion Cuff emphasizes that Bernard Baruch and the WIB did not dictate to big business. Rather, they were subjected to a variety of pressure groups and ideologies, including those of businessmen who hoped to utilize the war crisis to attain centralized industrial stabilization. Although the author is sympathetic to Gabriel Kolko's views concerning business-government relations he concludes that businessmen in World War I did not achieve the centralized institutional order that they sought and that other interest groups acted as countervailing powers.

Cuff has dug deeply in primary sources. He has examined the records of the WIB in the National Archives and dozens of manuscript collections relating to important political leaders and administrators. These materials he supplements with scores of newspapers and trade periodicals. Without doubt Cuff has produced the most authoritative volume available about the WIB and has filled a void of many years.

And yet even appreciative readers will not find this a wholly satisfying study. In part, it may be a matter of *Weltanschauung*. Cuff is so intent on demonstrating what was wrong with the WIB that he tells his readers very little about what was right. After all, despite many weaknesses, industrial mobilization in

the United States during World War I did achieve unprecedented goals. Its speed and magnitude completely confounded the predictions of the German General Staff, which had not expected effective American mobilization before 1920. Moreover, while Cuff does well in delineating some of the ideological considerations that governed war mobilization in the United States he hardly touches on the influence of technology in shaping government-business relations. The impact of ideology may be lessened when it is remembered that technological imperatives were operating not only in the United States but also in other industrial societies as diverse as England, France, Germany, and Japan, producing similar bureaucratic tendencies. The American experience must be measured by a worldwide rather than a purely national context. But if this is not a definitive work about the WIB it is the most informative yet to appear, and it can be read with profit.

GERALD D. NASH

University of New Mexico

ROBERT SOBEL. *The Age of Giant Corporations: A Microeconomic History of American Business, 1914-1970*. (Contributions in Economics and Economic History, number 7.) Westport, Conn.: Greenwood Press. 1972. Pp. xiii, 257. \$9.00.

General business history, as distinct from the study of corporate administration or accounts of single companies, is rapidly taking shape as a subdiscipline of American history. In the last two years the first comprehensive business text, two analyses of nineteenth- and twentieth-century structure and management, and Robert Sobel's "microeconomic" history from 1914 to 1970 have appeared. The latter is devoted to a rather detailed study of the external history of giant corporations, their adjustments to booms and depressions, their relations with government, and their mergers and conglomerations. For such information it is the best factual reference work that has appeared. Since the author has expressly avoided theory and large generalizations, the book is devoted chiefly to narration of events.

But no one can write history without generalizing, and the vulnerability of some of his conclusions illustrate the relatively primitive state of business historiography in which little

has been settled and there is no great store of established knowledge. A few examples may suffice. His interpretation of business in relation to the National Industrial Recovery Administration and General Hugh Johnson differs markedly from that of Louis Galambos in *Competition and Cooperation*. Sobel avoids the problems of the average American business, which is very small, but his generalization that "the message was clear: become large, merge or go out of business" (p. 210) is not borne out by the statistics showing rapidly increasing numbers of units right up to 1970. In part, his aphorism may come from the fact that he devotes much more space to manufacturing, which employs less than thirty per cent of Americans and is the chief area of concentration of control, than to any other type of business. The book also has a more optimistic quality than it might have had if written a couple of years later, a problem no author can guard against. The poor performance of the United States relative to the other leading industrial nations in both increase in income per capita and new consumer goods technology has suddenly become much more evident.

No author of a general book in a new field can avoid debatable interpretations, however, and they scarcely detract from the value and utility of Professor Sobel's book. It is a pioneer contribution to synthesizing the history of large companies since the First World War.

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GIUSEPPE FIORI. *Antonio Gramsci: Life of a Revolutionary*. New York: E. P. Dutton and Company. 1971. Pp. 304. Cloth \$8.95, paper \$3.45.

Antonio Gramsci (1891–1937), an impoverished, hunchbacked Sardinian intellectual, was a co-founder of the Italian Communist party and one of the principal victims of Fascist repression. He died in 1937 of tuberculosis and other complications aggravated by his ten years of imprisonment. Posthumously he has come to be regarded by the New Left as probably the most original interpreter of Marxism to emerge in Western Europe during the twentieth century.

The time is propitious, therefore, for this fine translation of Giuseppe Fiori's excellent

biography of Gramsci. The original Italian edition was brought out in 1966 by Laterza of Bari. Gramsci has found a well-qualified biographer. Fiori was born in Sardinia in 1923 and took a law degree at the University of Cagliari. Later he worked as a radio journalist and as a cinema critic for the chief Cagliari daily. He also traveled widely in Europe, writing for such Italian periodicals of radical orientation as *L'Espresso*, *Il Mondo*, and *Il Ponte*. He has also written two documentary novels about life in Sardinia and a study of contemporary Sardinian banditry, *La Società del Malessere* (1968). Fiori was in a much better position than any non-Sardinian to probe into and interpret Gramsci's island background. In addition to the interviews, Fiori consulted most of the published documentary sources relevant to Gramsci's career. While these are carefully footnoted in the Italian edition, for some reason they have been omitted in the English version, though it contains an updated portion of the original bibliographical essay. The index is confined to names of persons mentioned. Tom Nairn, the translator, is an Englishman. He has wisely recast complicated Italian prose into straightforward English, and he deserves particular praise for his adeptness in clarifying the often abstruse dialectical arguments and vocabulary employed by Gramsci—a mode of philosophical discourse that sometimes seems almost unintelligible to the English-speaking world.

The biography gets off to a somewhat slow start in Sardinia but rapidly becomes more absorbing as the author sympathetically describes Gramsci's cultural development at the University of Turin and his conversion to socialism during World War I. Fiori devotes careful attention to Gramsci's founding of the Turin newspaper *Ordine Nuovo* in May 1919 and his advocacy of the new "shop-steward committees" (*commissioni interni*) as the Italian revolutionary counterpart to the Russian soviets.

In May 1922 Gramsci was named Italian representative to the Comintern executive committee in Moscow. While there, he met and married a very neurotic young woman, Julia Schucht, who was to bear him two sons. His family life was to be awkward in the extreme.

On his return to Italy in 1924 Gramsci took over leadership of the Communist party from Amadeo Bordiga. The political differences be-

tween Gramsci and Bordiga, as well as with numerous other communist leaders, are very clearly delineated by Fiori. Although he had been elected to parliament, Gramsci was arrested by the Fascists in November 1926 and condemned to more than twenty years' imprisonment. During his years of incarceration Gramsci found himself increasingly at odds with the official policy proclaimed by the Comintern in 1928 (and backed by Palmiro Togliatti) that forbade any communist collaboration with Social Democrats in the revolutionary struggle against fascism. Gramsci was politically isolated; from prison he could not prevent his party from blindly obeying Stalin's orders. Meanwhile, he was also cut off ever more from his family. What sustained him through these tragic years was his reading and writing, carried out under the most trying conditions. The preservation and publication of his influential prison notebooks more than a decade after his death was to be a remarkable example of the triumph of mind over matter.

CHARLES F. DELZELL
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CHARLES P. LAROWE. *Harry Bridges: The Rise and Fall of Radical Labor in the United States*. New York: Lawrence Hill and Company, 1972. Pp. xi, 404. Cloth \$8.95, paper \$3.95.

This book is the product of a decade of inquiry into oral and written sources from Hyde Park, New York, to Hawaii. Bridges refused to be interviewed (although making available his union's records), criticized the book in manuscript, and blasted it after publication in a letter to the *New York Times Book Review*. Here, one would suppose, must be a study both thorough and critical of the most prominent labor radical of the past generation. I think not. To take small things first, the reader is put off by an abundance of typographical errors and by two different versions of an important quotation from Bridges (pp. 126, 351).

These minutiae reflect a more substantive carelessness. On those rare occasions when Larowe takes note of what was happening in the labor movement nationwide he is misleading and superficial, as when he characterizes the 1930s as a decade "without precedent" for its violence in American labor history (p. 32) or

feels obliged to apologize for antilabor actions by President Roosevelt (pp. 25, 106). Above all, *Harry Bridges* fails to come to grips with the problems posed by its subtitle: what kind of radical was Bridges, and why did the radicalism fade?

One can understand that Larowe would not wish to be the instrument for Bridges's deportation after the United States government had failed so many times. Yet Larowe is coy to the point of evasiveness about Bridges's relation with the Communist party. Let us suppose (although Larowe never says so in so many words) that Bridges did not belong to the party. Explanation is still required for the fidelity with which the union leader followed each twist and turn of the party line: softening his stand toward the New Deal in the mid-1930s, denouncing it during the "phony war" period from September 1939 to June 1941, and then proposing labor-management cooperation to win World War II. Too often Larowe is content to dispose of Bridges's politics by quoting his testimony on the witness stand. But it should be the business of the historian to tell the truth more clearly than a defendant in a trial is likely to be able to.

Similarly Larowe alludes—in a dependent clause within a sentence about something entirely different (p. 130)—to the fact that in 1936 Bridges promised the shipowners that his union would stop "using job action," that is, wildcat strikes. In the history of every CIO union the leadership's promise to repress unauthorized strike action by the rank and file was the single most critical step in the union's retreat toward conservatism. Murray and Reuther took this step in 1937, "radical" Harry Bridges in 1936. Why? If Larowe could explain this, he might be able to offer something better than old age or Michels's iron law of oligarchy in explaining Bridges's collaboration with the shipowners after World War II.

Perhaps the last word belongs to Bridges. "Collective bargaining is class collaboration," he testified at a deportation hearing. If by this is meant that the "fall" of radical labor is best explained by the conservatizing influence of collective bargaining itself, Bridges's own life exemplifies his words.

STAUGHTON LYNDE
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Complete Presidential Press Conferences of Franklin D. Roosevelt. Twenty-five volumes in twelve. Introduction by JONATHAN DANIELS. Volumes 1-2, 1933, pp. xvii, 407, 593; volumes 3-4, 1934, pp. 441, 304; volumes 5-6, 1935, pp. 395, 372; volumes 7-8, 1936, pp. 308, 212; volumes 9-10, 1937, pp. 467, 443; volumes 11-12, 1938, pp. 499, 326; volumes 13-14, 1939, pp. 484, 397; volumes 15-16, 1940, pp. 605, 395; volumes 17-18, 1941, pp. 423, 403; volumes 19-20, 1942, pp. 398, 316; volumes 21-22, 1943, pp. 416, 254; volumes 23-25, 1944-1945, pp. 279, 281, 121. [The pagination is that of the original transcripts; index to each volume is not paginated.] New York: Da Capo Press. 1972. \$40.00 each, \$450.00 the set.

The De Capo facsimile edition of twenty-five volumes of the press conferences of President Roosevelt, in twelve bound volumes, presents the raw materials of history in a readily available form to which students seldom have such ready access. Here is a direct copy of the press conference transcriptions, with all the typographical errors and inaccuracies intact, without benefit of editorial correction or notes and devoid of supplement beyond the index in each volume. The introduction by Jonathan Daniels is the only editorial contribution beyond the utterances of FDR.

It is right to call this collection a great mass of the raw material of history, and it is honest to acknowledge that much of it is very raw indeed and a lot of it not very material. But it will serve many a scholar until an annotated version appears, with the guidance that can be given by editorial footnotes and chronologies. No one will write a history of this period without consulting it, as surely as no one will write an adequate history of the period by consulting it alone.

This is a unique document that records a unique institution in American government—the Roosevelt press conference. There was nothing like it before in American government. Since the administration of Harry S. Truman there has not been anything remotely resembling it. It is not likely that there will be anything just like it in the future, because the exact combination of circumstances that existed from 1933 to 1945 are not likely to be repeated. The sheer size of the press gallery began to alter the Roosevelt sessions toward the end. (As Jonathan Daniels notes in the preface there were only 279 accredited members of the press

galleries in the twenties, while at Roosevelt's death there were 695 active members of the gallery, plus a new radio gallery of 102, and 89 accredited photographers.) Once President Eisenhower opened the press conferences to television nothing remained of the FDR format, with its cozy off-the-record and background rules. History, moreover, despite its predilection for repetition, is not likely to repeat the atmosphere of the Roosevelt years, when a president could count on newspapermen as allies, first in a war against depression and then in a war against the Nazis and Fascists.

It is not wise to measure the communicativeness of governments by the number of press conferences held, as some have been tempted to do. There are press conferences and press conferences, and Roosevelt's press conferences were as notable for the skill with which he avoided communication when it was not convenient as they were for the effectiveness with which he communicated when he wished to do so. Roosevelt held 998 press conferences, Truman 322, Eisenhower 193, Kennedy 64, Johnson 126, and Nixon 23 in his first thirty-nine months.

To his contemporaries Roosevelt seemed to present at his press conferences the figure of a public man of very liberal inclinations, bold and venturesome and even indiscreet; but to read these pages now (in the light of what has since transpired) is to gain a rather different image—the image of a man who was essentially conservative, cautious on some occasions even to timidity, and respectful of current opinion as long as it seemed likely to prevail.

These transcripts show how cautiously he moved to disclose his support for a whole string of positions on which he later became articulate: guarantee of bank deposits, Nazi persecution of Jews, lynching, federal aid to education, intervention in Europe, deficit spending, veterans' benefits, the United Nations, the recognition of de Gaulle, the treatment of postwar Germany, and the Italian settlement.

His reputation for candor with the press rests more on the manner with which he managed the press conferences than on the meat in them. He took the nation, step by step, toward a military confrontation with the Axis powers

without once acknowledging that we were drawing closer to war until after Pearl Harbor. He prepared the nation's armed forces for war while events prepared the mind of the country for that eventuality. Both were ready when the time came. The brightest and best brains did not always give him the best advice, as he ruefully acknowledged when the steel crisis arose to threaten military preparations.

He was most adroit, and least candid, in fencing with reporters over the third term and the fourth term. He did not hesitate to brand reporters as dunces for quite legitimately inquiring about his intentions. His evasiveness exhausted the psychological hurdle of a third term before the campaign even started. He skillfully refused to be drawn into "politics" in campaign year press conferences and smothered the press with lectures on statecraft and preparations for the nation's defenses.

He was least adroit in the Supreme Court fight, in which he was so slow to claim a victory that it was put down as a defeat. He used the utmost skill in easing New Deal figures out of government without a fuss, allowing no questions on impending changes to ruffle him.

Even to those who were present at many of these press conferences the written record presents a picture of Roosevelt's comradery with the administration supporters in the gallery and of his hostility toward the press in general that is surprising. He adroitly established a sense of rapport with reporters, identifying himself with their work and burdens. His censure of the press was more severe than anything that has come out of the White House (or the government) since, but it was mostly off the record. He blamed the British press for the failure of the London Economic Conference in 1933, when the real blame was his own. He loved to philosophize about the press and fancied himself a newsman because of his experience on the *Harvard Crimson*. He said (November 7, 1934) that columnists writing once a day had to write "a lot of pure bunk." He thought "newspapers have a lot less influence than they had fifteen or twenty years ago." He accused Ernest Lindley of lying about his intervention in New York City politics in a furious and intemperate press conference on August 9, 1937. He roundly lectured the American Society of Newspaper Editors in April

1938. He said the phrase "newspaper story" was coming into general use. He complained: "There is not a newspaperman that comes into my office that understands the ramifications of the national problems." He said eighty-five per cent of the daily press had been inculcating fear in the people. On the occasion of a story about a bomb sight he asked (February 7, 1939) "is it a patriotic thing, if we have a secret of this kind, to blazon it out for the benefit of foreign governments?" In October 1942 he accused a "minority" of the press of "giving out sententious views—news stories that 'just aint so.'" On December 18, 1942, he asked Earl Godwin to give John O'Donnell an iron cross for a story he had written. On June 29, 1943, he accused reporters of coloring the news and said many of them wrote in accordance with orders from their editors. He said stories about WAAC morals were a "newspaper job" that was "shameful" and "hurt the war effort." On August 31 he said a Drew Pearson column was "detrimental not only to the foreign relations of the United States but to the unity of the United Nations, and therefore the winning of the war." He said Pearson was a "chronic liar" and "not the only one" in the press.

As the war proceeded Roosevelt threw a veil of secrecy over more and more of the government's operations, excluding the press from conferences and consultative gatherings like the food conference. The press conferences became less and less productive of real news. The president became more and more sensitive about stories implying division within the government or between the United States government and its Allies.

One is struck by the shadows of things to come. After the repeal of Prohibition he was asked about violators of the dry laws. He said: "Well they violated the law." Would there be amnesty? "The question of the reasonableness of the sentence is a different thing but they certainly violated the law. There is no getting around that." He warned in 1935 that the country was headed toward control of oil. He warned on January 23 that executive responsibility for foreign affairs was limited only by the power to make treaties and by appropriations. He talked about mass-produced houses in 1936. He talked of presidential powers to "impound" appropriated funds in February

1942 but said he was, at the same time, carrying out the obligations imposed by Congress. In 1937 he quoted Woodrow Wilson as saying "once an appropriation is made or a law is passed, the appropriation should be administered or the law executed by the executive branch." And Roosevelt added his own interpretation of this: "Once an appropriation is made, I have a mandate from the Congress to carry it out. No question there."

In 1940 Roosevelt faced the guns or butter, defense or welfare arguments in an endless press conference with National Youth Administration spokesmen. He met the complaint that he was spending a billion more for arms and a billion less for relief, the youth, and the unemployed. He had a hard time persuading his questioners.

There is hardly a subject of current political or economic interest to which reference cannot be found in these volumes. In the end, though, they tell less of history than of the man who made it in these fateful years. These pages say better than any that have been written that he was a man of great courage, great good humor, and indomitable faith.

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REXFORD G. TUGWELL. *In Search of Roosevelt*. Cambridge, Mass.: Harvard University Press. 1972. Pp. ix, 313. \$12.95.

REXFORD G. TUGWELL. *Off Course: From Truman to Nixon*. New York: Praeger Publishers. 1971. Pp. ix, 326. \$7.95.

During the early years of the New Deal, Rexford G. Tugwell was a prime target of Roosevelt's right-wing opponents. He was made out to be an ultra New Dealer, a prototype for the countless caricatures of the professor in Washington who, never having carried a precinct or met a payroll, was bent upon ruining the nation with radical nostrums. Now, forty years and many books later, Tugwell is secure in his reputation as one of those brilliant, innovative social scientists who helped move the nation toward more vigorous interaction between the government and the economy. He has not abandoned his faith in government economic planning, the "collectivism" for which he stood in the 1930s, and in both his two latest books

he vigorously defends it. Further, in his writings he has been one of the most perceptive interpreters of Roosevelt. Although he has published extensively on these themes he still has much to say that is thought-provoking. That is especially true of *In Search of Roosevelt*, a collection of the best of the essays he has written during the years since Roosevelt's death. Most of them appeared in journals not easily available; two appear in print for the first time. The most important are two reprinted from the *Western Political Quarterly*, "The Compromising Roosevelt" and "The Experimental Roosevelt," which are in part a reminiscence and in part an exploration of the fascinating problem of means and ends. Thus, Roosevelt's approach to legislation during the "hundred days": "He often felt that if many panaceas were authorized in a measure, the sponsors of each would at least not object; and it would be accepted by the Congress with a minimum of argument or delay. Once the bill was passed one or another or maybe several of embodied schemes could be tried and discarded if they failed. So far as Roosevelt was concerned, the scheme was of no importance, however much it meant to its doctrinaire authors. He was interested only in results, and grand results at that" (p. 286).

Along with his exploration of basic motives and techniques Tugwell presents Roosevelt as a flesh-and-blood figure. He not only explains the significance of the meeting of the president-elect with President Hoover over the war debts question late in 1932 but also recounts Roosevelt teasingly insisting to Raymond Moley, who was to accompany him to the White House, that Moley must wear striped trousers. Again, in a vivid account of Roosevelt's ventures in Georgia farming, Tugwell sets forth numerous homely anecdotes to arrive at a significant conclusion. Far from operating the farm as a frivolous hobby, as it appeared to be, Roosevelt was demonstrating how to rehabilitate a worn-out cotton growing area into one of tree farming and beef production.

There is less of Roosevelt's human qualities and his fallibility in *Off Course*, which is a tract on the developments of the past quarter century. Roosevelt becomes the model against which to measure the failure of subsequent presidents to move in domestic policy toward

collectivism and in foreign policy away from cold war.

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FRANCIS V. O'CONNOR, editor. *The New Deal Art Projects: An Anthology of Memoirs*. Washington: Smithsonian Institution Press; distrib. by George Braziller, New York. 1972. Pp. ix, 339. \$12.50.

The memoirs in this well-illustrated anthology were commissioned in 1968 as part of a study of the cultural and economic effectiveness of the New Deal Art Projects—a study commissioned by the National Endowment for the Arts and conducted by Francis V. O'Connor. Because he sought persons who could discipline recollections with archival research, O'Connor has produced a volume that should provoke interest among scholars as well as nostalgia among project alumni. Although the three Treasury Department programs, routinely described by Olin Dows, were nationwide in scope, as was the WPA Federal Art Project (WPA/FAP), it is the vibrant, trouble-ridden New York Art Project to which O'Connor has turned for his writers. And well he might for New York provided on a grand scale the triumphs and hazards of this federal foray into subsidized art.

The director of the New York region was Audrey McMahon. Referred to by her assistants as "arsenic and old face," this formidable woman fought for her artists with consummate skill and dedication, revealing in her essay the intermixture of art, relief, bureaucracy, and politics that so characterized all of the WPA cultural projects. The artists themselves describe in greater detail the functioning of the various creative divisions. Edward Laning, in a delightfully informative account of his development as a mural painter, provides extensive information on his Ellis Island murals as well as those in the New York Public Library. Joseph Solman furnishes similar insights into the working of the easel division, which employed such colleagues as Joseph Stella, Marsden Hartley, Milton Avery, Ad Reinhardt, Jackson Pollock, Mark Rothko, Jack Levine, Philip Evergood, Robert Gwathmey, Ben Zion, and Bryon Browne. Emphasizing the

diversity of their art, Solman effectively challenges the stereotype of federal artists as primarily producers of the hackneyed proletarian art so often associated with American scene painters in the thirties and with WPA painters in particular. The point is underscored by Rosalind Bengelsdorf Browne who describes the interrelationship between the American Abstract Artists and the WPA/FAP. When the Museum of Modern Art was still showing American Regionalists the government was also exhibiting pioneer abstractionists, sponsoring welding demonstrations by the sculptor David Smith, and encouraging Ashile Gorky in his now destroyed abstract murals for the Newark Airport. The Graphic Arts Division pioneered in its own way according to Jacob Kainen. By expanding the technical possibilities of the media, especially in the area of lithography, and by enlarging popular interest through its vast allocation of prints to public institutions, the FAP, Kainen argues, effectively bridged the gap between the moribund etching societies of the past and the virtual explosion in the graphic arts occurring in the last two decades. Less innovative but no less noteworthy was the Index of American Design. A part of the rediscovery of America in the thirties, these illustrations of historic American crafts reveal "the existence of an ingenious and highly respectable tradition of genuine spontaneous creativity early in our history." Lincoln Rothchild's claim is not an inflated one as anyone who has examined these meticulously executed plates can attest.

In sum, what O'Connor's contributors have done is to provide an informed and balanced assessment of selected aspects of the federal government's involvement in the visual arts. In the process they make no effort to minimize problems confronting this relief-born enterprise. Yet in a curious way theirs is also a story of partial triumph—triumph, in the main, of artistic freedom; of democratic practices in an elitist profession; of technical and esthetic growth in the midst of economic insecurity; of a vision of "arts for the millions"; of an unprecedented cultural democracy at a time when all democracy was threatened. On each of these themes O'Connor could have elaborated more fully in his introduction. His anthology, nonetheless, is a welcome addition

to the growing literature on the cultural history of the depression decade.

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EDWARD L. and FREDERICK H. SCHAPSMEIER.
Prophet in Politics: Henry A. Wallace and the War Years, 1940-1965. Ames: Iowa State University Press. 1970. Pp. xv, 268. \$8.95.

The tragedy of New Dealer Henry A. Wallace was that while he raised crucially important foreign policy questions during the cold war and spotlighted the neglect of the nation's Negro and the poor, he allowed himself to be co-opted, at a critical period, by Communists and fellow travelers and thus appear as an apologist for Communist tyranny and aggression.

As FDR's third-term vice-president, and head of the Board of Economic Warfare, he fought vigorously for experimental development and stockpiling of scarce resources prior to and during the war years. With a unique rearing in scientific farming and recent mastery of Spanish and Russian he made a significant impact upon governmental officials and farmers during important wartime missions to Latin America, Russia, and China. His religioeconomic interpretation of events—his stress on social justice, the brotherhood of man, and the blessedness of universal peace—attracted the overwhelming support of rank-and-file Democrats nationally. Though Southern leaders and urban Democratic bosses convinced FDR to dump Wallace for the 1944 race, this prophet from Iowa came within a few votes of winning renomination. Might America have played a fundamentally different role had FDR defied his conservative advisers?

As secretary of commerce, Wallace lamented America's bipartisan foreign policy, concluding that we were on a collision course with the Soviet Union. Instead, he suggested a return to a more moderate approach toward Moscow because of his belief that the Soviet government and its leaders had disclaimed aggressive intentions or desire for world domination.

Truman's offhand endorsement, in advance, of Wallace's controversial foreign policy ad-

dress of September 1946 not only demonstrated a lack of sophistication on the part of FDR's successor but evoked a major cabinet crisis that was finally settled when the chief executive fired him. Gradually, thereafter, Wallace veered away from the liberal, democratic mainstream. During his brief reign as editor of the *New Republic* magazine he became increasingly isolated from the real world by fawning Communists and fellow travelers. Michael Straight, the magazine's publisher, informed me in 1947 that he found it impossible to break through the human barrier surrounding Wallace for private conferences. After Wallace fulfilled a number of speaking engagements in Britain during the spring of 1947, Labourite left-winger Jennie Lee bemoaned his "presentation of world affairs in which Russia is always right and the rest of the world always wrong."

Bearing the mantle of FDR, Henry Wallace was in a position to exert tremendous influence upon millions of Americans who still recalled the New Deal with warmth and affection. Instead, as presidential candidate of the Progressive party during the 1948 race, he threw in his lot with the controlling authoritarian Left and momentarily besmirched his role in American history as an apologist for the Soviet Union.

In this intriguing and scholarly study, the second of their two-volume biography, the authors have filled many gaps and disposed of many myths. Though neglecting, for example, to distinguish clearly the role of the Liberal party in New York, which had resulted from defections from the American Labor party because of the latter's domination by Communists and fellow travelers, the authors, nevertheless, have placed Wallace in the proper perspective. He was a farsighted botanist, philosopher, and muckraking journalist who became not only an outstanding public servant but also a spokesman for discriminated minorities and urban laboring masses. However, he "was a man driven by conscience and conviction, so much so that his concern for political realities was often obscured by the cause itself."

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NORMAN D. MARKOWITZ. *The Rise and Fall of the People's Century: Henry A. Wallace and American Liberalism, 1941-1948*. New York: Free Press. 1973. Pp. xi, 369. \$8.95.

Past studies of Henry A. Wallace have usually stressed the "agrarian years." Wallace as agricultural reformer, they have concluded, was innovative and effective. But in the 1940s, as he became embroiled in urban and "popular-front" politics, his befuddled mysticism and naïveté made him a pitiful and tragic figure.

Markowitz's study attempts both to reassess Wallace and fit him into a larger drama of wartime visions and postwar tragedy. Drawing upon massive research in the Wallace papers and related collections it focuses, in particular, upon how the wartime vice-president became the leading spokesman for a "World New Deal" or "people's century" and how, step by step, the chances of implementing this vision melted away. From the beginning, Markowitz argues, Wallace and other "social liberals" failed to see the contradictions in their conception of a "progressive capitalism," and once wartime compromises and lapses had left them to work within a structure of corporate power, pressure-group politics, personality worship, and misconceptions about Russia the outcome was virtually inevitable. Freed from threats of mass unemployment and fascist aggression, a resurgent conservatism could not be contained. Delivered into the hands of a "bumbling" Harry Truman, the Roosevelt coalition disintegrated. And after 1946, as Truman reacted with a blend of anti-Communist rhetoric, populist poses, and economic imperialism, many "social liberals" lost their "nerve." Sacrificing ideals for "toughness" and invoking images of a "Red-fascism," they became "cold-war liberals," joined in crushing Wallace's courageous defenders of the earlier vision, and helped usher in Acheson's "geopolitical fantasies" and McCarthy's Red-baiting.

As a reinterpretation of the 1940s Markowitz's work is more provocative than convincing. It should stimulate considerable rethinking. But on balance its portrayal of the Truman administration comes too close to caricature, its reading of Russian-American relations rests upon too many questionable premises, and its categories of "social," "corporate," "interest-group," "popular-front," and

"cold-war" liberals often distort as much as they clarify. As a careful reconstruction of Wallace and his activities, however, the book is more successful. Its accounts of the clash with Jesse Jones, the vice-presidential nomination of 1944, and the Madison Square Garden speech of 1946 are the best in print. And while one may question the "realism" of Wallace's dream, even in a "democratic socialist America," the study offers convincing evidence that the dreamer was neither the "visionary crackpot" or "Communist dupe" of orthodox history nor the variant of "Open Door imperialist" seen by some New Leftists. In future accounts of the 1940s it seems likely that he will be depicted as more sophisticated, more courageous, and less foolish than he has been in the past.

ELLIS W. HAWLEY

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Foreign Relations of the United States. The Conferences at Washington and Quebec, 1943; The Conference at Quebec, 1944. (Department of State Publications 8552 and 8627.) Washington: Government Printing Office. 1970; 1972. Pp. xcv, 1382; 1, 527. \$7.00; \$4.75.

Foreign Relations of the United States defies reviewing. Given the amount of time one can reasonably spend on such a job it is simply impossible to do the two things that should be done with any documentary collection—that is, fully evaluate the fullness and fairness of the selections and point out what is new and useful information for scholars. Both of those tasks are tantamount to preparing a fully researched monograph on each major subject dealt with in each volume under review, and in this case that would amount to at least eight books. This review is, then, like almost every other of this series I have ever read—vague, impressionistic, and all too uncritical.

The materials in the volume titled *The Conferences at Washington and Quebec, 1943* have been available to scholars since 1970 and have already begun to appear in articles and monographs. The two conferences, probably better known by their code names of TRIDENT (Washington, May 12-25, 1943) and QUADRANT (Quebec, August 11-24, 1943), mark the point at which postwar political

and economic considerations first began to muddy the waters for the Americans. To be sure, as the large number of military documents indicates and as the editors point out in the introduction, military planning dominated the conferences, particularly the one at Washington in May, although the troublesome issue of Soviet-Polish relations did crop up at the TRIDENT meeting. Three months later, at Quebec, there seemed to be a far deeper concern regarding the structuring of the postwar world. The terms of the Italian surrender, British access to atomic energy with its potential for postwar commercial use, German and Italian territorial questions, and the problem of a postwar international organization all appeared in either the actual discussions or the preconference briefing papers. In the military talks familiar themes appear. As outlined in his memoirs Churchill began at Quebec a fervent campaign to commit the United States to a military effort in the Dodecanese Islands—a campaign that would have eventually forced a re-evaluation of the date for the Normandy invasion set a few months earlier at the TRIDENT conference. As usual, the demands of the European Front prevented any meaningful shift of resources to the Asian and Pacific theaters, in spite of a series of vague and eventually unfulfilled promises.

The volume titled *The Conference at Quebec, 1944*, though substantially shorter than the one just discussed, is somehow more intriguing. The reason is, I suppose, that politics and postwar planning dominated the talks. It was at the Quebec meeting that Treasury Secretary Morgenthau presented his plan for the partitioning and pastoralization of Germany. Clearly recognizing the relationship between reparations, reconstruction, and any re-ordering of the main thrust of the German economy Morgenthau argued that the reconstruction of Germany was both dangerous and politically unpopular in the United States (an appeal well calculated to sway Roosevelt), hence large reparations had to be eliminated. The details of the argument and the supporting documentation are set forth in full, and it appears that a re-evaluation of the Morgenthau plan may well be called for. Other concerns also characterize the OCTAGON

conference. Most striking was the clear and very deep concern over the development of tension between the Western Allies and the Soviet Union. The struggle for power in Europe clearly stimulated such suggestions as an occupation of Norway as well as Poland, and the various briefing papers are replete with indications of the awareness on the part of political advisers that a crisis seemed in the offing.

Both volumes are examples of the best the *Foreign Relations* series can offer. In each case the Historical Office has assiduously searched out relevant materials in other collections, particularly in the Franklin D. Roosevelt Library, and has also provided informative references to various printed memoirs that add additional information. The footnotes invariably answer the identification and technical questions, while the maps and photographs in each volume add a touch of life.

In a sense, however, such extensive research and work outside the Department of State records only demonstrates the shallowness of the conception behind the entire series. Put in its simplest terms, foreign policy is not and never has been the exclusive province of the secretary of state and his department. In order to assemble a reasonable sampling of the documentation behind modern American foreign policy the editors are forced to examine a wide range of non-State Department materials. The role of the military, obviously critical in America since 1941, requires at least the papers of the Joint Chiefs of Staff. Since that organization's attitude toward declassification is far more conservative than that of the State Department (which itself is hardly liberal except by comparison to other governments) one can be assured that what appears is carefully screened; nor can independent scholars examine those papers in order to evaluate the work of the editors of *Foreign Relations*. Other governmental organizations—the Commerce Department, the Treasury Department, the Departments of War, Navy, and later Defense—all play a critical role in various aspects of foreign policy, yet precious few of their documents appear within these or subsequent volumes of *Foreign Relations*. That the staff of the Historical Office cannot cope with such a mass of material is obviously true—but hardly an argument for the completeness of

the series. Even if the editors had the personnel, some invaluable collections are simply not available. The Stettinius papers were closed to those editing the volume on the Quebec Conference of 1944, yet Stettinius was a prime mover in the State Department as of the fall of that year. Later volumes have suffered because of the unavailability of the Truman papers, and we can expect similar problems in the future with other presidential papers. Can you imagine a volume on American relations with China for the Nixon years without full, complete, and open access to the files of the Kissinger office in the White House? The need for full access to presidential records is driven home by an examination of the volumes under review. In each there is a minimum of documentary evidence on the actual proceedings of the conferences between Churchill and Roosevelt. Without the vast number of briefing papers, later memorandums, and other materials gathered by the editors (particularly those in the Franklin Roosevelt Library) we would have almost no real knowledge of what actually transpired. Perhaps the most telling argument along this line is the fact that no monograph is ever considered as adequately researched if it relies solely on *Foreign Relations of the United States* for its official documentation of foreign policy matters. Scholars are always and rightfully expected to examine all the records themselves. If one follows that idea to its logical conclusion, then the series is of use primarily for the training of students, to provide background in nondiplomatic history studies, and as a guide to further research. Does that really justify the time, expense, and effort? This is not the place for a re-evaluation or proposed reshaping of the series, but I am reminded of Louis Morton's call for the establishment of a Historical Office outside and above the Department of State. Certainly one thing seems clear. Our current all-too-restricted view of what constitutes the history of American foreign policy and diplomacy has been, at least in part, caused by the profession's overexuberant praise for the *Foreign Relations* series as a primary source.

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FORREST C. POGUE. *George C. Marshall: Organizer of Victory, 1943-1945*. Foreword by OMAR N. BRADLEY. New York: Viking Press. 1973. Pp. xviii, 683. \$15.00.

In this third volume of his magisterial biography Forrest Pogue reaches his stride as a biographer of the first rank and General Marshall emerges as one of the dominant figures of the Second World War, the worthy antagonist of Churchill and the main support of President Roosevelt—in a real sense “the organizer of victory.” This is biography at its best. With the two preceding volumes and the prospect of two more still to come we have not only the most definitive and authentic but also one of the most comprehensive portraits of any leader of the wartime period, a work comparable in breadth and meticulous attention to detail to Douglas Freeman's biography of Robert E. Lee.

Marshall's public life was plainly exposed to view, and though he repeatedly refused to write his memoirs he did talk at length to Mr. Pogue. But Marshall, always “reticent about his personal life,” submitted reluctantly to Pogue's “probing of his private thoughts and emotions” (p. xiii). As a result Pogue had to depend on Marshall's associates and friends for whatever anecdotes and stories he could pick up to portray the human side of his subject. Despite his heroic efforts, however, there is little in the book about the general's life and thoughts and virtually nothing about his home life or family relations.

On the other hand, Pogue has little difficulty portraying Marshall the soldier and public figure. Drawing on Marshall's correspondence, on a wide variety of contemporary accounts, and on more than three hundred interviews with Marshall's wartime colleagues Pogue documents in full detail the general's career and the qualities that earned him the respect and admiration of all with whom he came into contact. *Time*, which named Marshall “Man of the Year” in 1944, said he was “the closest thing to the indispensable man,” and under Pogue's masterly hand Marshall more than lives up to this characterization. Stimson thought him the finest soldier he had ever known, and Robert Lovett called him a magnificent human being. His integrity and self-denial were remarkable. He denied all pub-

licity for himself unless it contributed to the war effort and refused to use his influence for personal gain or friendship. He had an unerring judgment of men, and his ability to select the best man for the job, wrote General Bradley, was almost uncanny. Fiercely independent, invariably truthful, and remarkably free of ambition he exercised enormous influence with Congress and the American people. His grasp of complex subjects, his mastery of detail, the clarity and eloquence of his presentation, and his commanding presence were enormously impressive. Invariably he was compared to George Washington. "His integrity," wrote Douglas Freeman, quoting Jefferson, "was most pure, his justice the most inflexible I have ever known, no motives of interest or consanguinity of friendship or hatred being able to bias his decisions" (p. 358).

Despite Pogue's disclaimer, this volume is more than biography; it is a first-rate history of the two climactic years of the war viewed from the perspective of one who participated in all the major Allied decisions and who, possibly more than any other, shaped the course of the conflict. It is as broad and complete a view of the Allied war effort as one could hope for, since Marshall's responsibilities encompassed virtually every aspect of the war: strategy, manpower, procurement, production, organization, and even morale. The forces that he had built, the weapons he had assembled, and the plans he had made since 1939 reached fruition during these two years.

Appropriately, the volume opens with the Casablanca Conference in January 1943 as the initiative was slipping away from the Axis. The decisions made then set the stage for the measures that led ultimately to victory in Europe—the combined bomber offensive, the drive up the Italian boot, the cross-channel attack, and the landings in southern France. As Pogue recounts the story of these years, they were not years of unbroken triumphs and resounding success for Marshall. There were setbacks on the field of battle; differences over strategy and command with the British, with the Navy, and with MacArthur; personal tragedy in the loss of a stepson for whom he felt a father's love; and professional disappointments. Of these, the greatest was the failure to secure the coveted command of the Allied

invasions of Europe, the culmination of all his plans and hopes. Pogue tells the story of this decision, the greatest drama of Marshall's life, more completely and in more detail than it has been told before.

Inevitably, this volume will be read in the light of the charges of treason made during the McCarthy era concerning the agreements reached at the Yalta Conference and the decision to leave Berlin and Prague to the Russians. Pogue deals with both charges and finds no merit in them—at least so far as Marshall is concerned. The Army Chief of Staff, as well as the other U.S. chiefs, he says, played a comparatively minor role in the agreements reached with Stalin, restricting themselves to the judgment that in the event it proved necessary to invade Japan it would be desirable to have Soviet assistance. Marshall understood politics very well indeed, but he was extremely careful, Pogue points out, always to confine his advice to the president to military matters. Speaking of the military, he once wrote, "We have a great asset and that is that our people, our countrymen, do not distrust us and do not fear us. . . . We are completely devoted, we are a member of a priesthood really, the sole purpose of which is to defend the republic" (p. 458).

In a sense this volume is incomplete. It ends with the defeat of Germany in May 1945, omitting the story of the closing months of the war. Pogue's decision to omit the story of Japan's defeat was made on the ground that these events were needed as background for Marshall's mission to China, to be covered in a later volume. There is room for disagreement here, but there is no doubt that the result of this decision is to weaken the present volume, probably the keystone volume in the series, and to diminish its value to the student of World War II.

Despite this reservation the third volume of the Marshall biography stands out as one of the best works yet written about the war. It is global in scope, deeply researched, thorough, written with clarity and forcefulness, and amply supported with photographs, maps, a chronology, a bibliography, and detailed notes. And not least among its virtues is the perspective it provides for those whose view of the military has been shaped by the war in Vietnam. No

reader of this volume can fail to be struck by the contrast between the attitude toward the American soldier in Vietnam and Marshall's fierce pride in the GI of World War II, or the "body count" of Vietnam and Marshall's strong feelings about casualties. His deep concern for the welfare of the ordinary soldier, his careful selection and watchful supervision of commanders on whom depended the lives of the troops, and his enormous prestige are timely reminders that in a different time and in a different war the army was the agent of the popular will and its leaders the heroes of a generation of veterans.

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MARGARET TRUMAN. *Harry S. Truman*. New York: William Morrow and Company. 1973. Pp. 602. \$10.95.

SUSAN M. HARTMANN. *Truman and the 80th Congress*. Columbia: University of Missouri Press. 1971. Pp. viii, 241. \$9.00.

Two more books on what Mrs. Daniel calls the "Truman Years." One can only feel morose that historians and relatives alike seem agreed on the term. Even I. F. Stone succumbed to the temptation. Yet after reading these two books one is bound to conjure with the possibility that the significance of the Truman presidency lies essentially in the fact that the fate of the United States and of the world was for a while in the hands of a man of limited sophistication, a man whose mental frontiers seemed bounded by the operations of Jim Pendergast on the one hand and General Douglas MacArthur on the other. Between the two he could and did make a choice. We may rejoice that Truman's choice was what it was—because maintaining purity in domestic American politics was less important than refusing to challenge the Yellow Peril beyond the Yalu River. But to imply that he somehow dominated these years is to give history a hyperbolic distortion. Let us be content to say that we were lucky; it could have been worse, except for the inhabitants of Hiroshima, Nagasaki, and North and South Korea.

Margaret Truman's book is a daughter's *apologia pro vita mei patris*. It begins, understandably, with the spectacular electoral tri-

umph of November 1948 and then starts over again to recount "Dad's" political career from Missouri courthouse politics and highway building to the vice-presidency and after. Despite her unvarnished defense of her father's cold-war policies and her very sparse and lenient treatment of his internal security and civil rights programs, there is much to be learned from Mrs. Daniel's book. Not least are the insights into one of the most important aspects of twentieth-century American social-political relationships. That is the tension between the "Eastern establishment" and the political professionals of the Midwest and Southwest. To read the Trumans' assessment of the Roosevelts is like reading the Johnsons' assessment of the Kennedys—and no doubt the Nixons' views would concur in both cases. Middle America, while not a geographical expression, seems to have a geographical weighting. Mrs. Daniel's comments are precise: "Wooing his enemies with gifts was one of Mr. Roosevelt's favorite tactics. My father never believed it was good politics and frequently said so." In 1940 some Democrats believed that a third term was another Roosevelt innovation that was splitting the party and his daughter reports: "My father, with his respect for American political tradition, felt the same way." The resentment deepened when FDR tried to buy Truman away from running for re-election in 1940 by offering him a life appointment to the Interstate Commerce Commission. References to "Roosevelt yes-men," to FDR as "ever the astute politician," to Harold Ickes as a "Roosevelt prima donna," to "Mr. Roosevelt's inability to pass on responsibility," and to the Roosevelt "Palace Guard" seem to add up to repayment for the gleeful charges that Truman was the "Ambassador in Washington for the defunct principality of Pendergastia."

Beyond these unguarded intimations there are some truly hair-raising points of emphasis. Truman's conviction that he was "very close to war with Russia" in 1948 is further documented. But even more important is his daughter's stress on his interpretation of NSC-68—the National Security Council policy review of 1949. "Although few of his biographers have noticed it," writes Mrs. Daniel, "he specifically rejected the policy of containment. 'Our purpose was much broader,' he said, 'We were

working for a united, free and prosperous world.'"

Professor Hartmann's study of Truman's relations with the Eightieth Congress is a superbly researched book, tightly packed with voting records and yet firmly interpretive. Starting with the unhappy congressional electoral results of 1946 she sets the stage with yet another statement of the social-political tension so often implied by Mrs. Daniel. In this case it is the specific disaffection of the "highly articulate group of middle-class reformers" who had worked for FDR. Seeking in Truman "a new source of unity and inspiration" they found him spectacularly wanting—"the small-town, midwestern, machine politician with the country accent and adventurous grammar could hardly fill the shoes of a sophisticated, charismatic, and seemingly independent statesman." Inept presidential leadership during the Seventy-ninth Congress, the failure of reform legislation, "the widening breach with the Soviet Union and U.S. support for reaction and imperialism abroad" all led to dark despair.

Professor Hartmann traces with exhaustive precision Truman's period of "appeasing" the Eightieth Congress, his success in getting a consensus on foreign policy, and the means by which (with the invaluable aid of Clark Clifford) he forced the Congress to build its famous "record." Professor Hartmann is harsh in her judgment: "His primary objective was not legislative results, but the building of a record against the Eightieth Congress for the 1948 campaign. . . . In bombarding Congress with requests that were certain to be denied, Truman cast himself as the Galahad of reform pluckily fighting the dragon of privilege." At the same time the president scored off the liberals by condemning Wallace's "insidious propaganda" for peace, declaring "I will not accept the political support of Henry Wallace and his Communists." In March 1948 he ignored General Marshall's plea to avoid inflammatory language and publicly charged the Soviet Union with sole responsibility for obstructing world peace.

Despite the sharpness of her scholarly scalpel Professor Hartmann concludes her important book with an essentially moderate assessment: "Given the conservative nature of U.S. politics, which the President himself symbolized,

the general tendency of domestic reform to support the long term needs of corporate capitalism, and the limitations on presidential power, and taking into account the long gestation period of many Populist-Progressive goals, Harry Truman's contributions to domestic reform were substantial."

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MARIAN J. MORTON. *The Terrors of Ideological Politics: Liberal Historians in a Conservative Mood*. Cleveland: Press of Case Western Reserve University. 1972. Pp. xi, 192. \$5.95.

In this doctoral dissertation presented at Case Western Reserve University, Marian Morton carries onto new ground the furious attack New Left scholars have been making against the so-called "consensus" historians of the post-World War II era. Instead of disputing their conclusions, she has concentrated intensively and exclusively on their underlying premises. Her subjects are Arthur M. Schlesinger, jr., Louis Hartz, Daniel Boorstin, Edmund Morgan, and Richard Hofstadter. These men, she argues, failed in their own early intention of giving a more effective and coherent form to American liberalism; they failed because they lost the faith in reason that distinguished their progressive forebears. Her study is both a manifesto for the more rationalistic strain in the New Left and an exploration of the intellectual history of the last three decades.

The most fresh and interesting aspect of the book is its demonstration that the five historians under scrutiny tended to move in the same direction under the impact of McCarthyism. Already deeply influenced by a realistic, nonrational view of human nature derived from modern social science, the liberal historians were trying to find a balance between realism and idealism. McCarthy's assault on intellectuals upset the balance by bringing home the destructive effects of ideologies. The historians then embraced a stultifying realism that has downgraded the intellectual and depreciated the power of idealists to make a better world. Although Morton calls me an apologist for liberal history, I think her criticism is in general outline correct, and I hope the next generation of scholars will do more justice to the place of reason in history

than has the last. But nothing will be gained and much lost if the danger of ideological passions is forgotten and we fall to cutting up one another with logic-chopping meat axes. Morton's analysis—shrewd but ruthless—often squeezes her subjects to fit a conceptual scheme and to force confessions from their lips. Thus Hartz becomes a spokesman for capitalism, and Morgan a proponent of worldly self-interest, while Hofstadter endorses the mental technician as the preferred type of intellectual. These painful distortions culminate in the last chapter in the charge that the liberal historians "have committed the intellectual sin of writing a history which they cannot have believed in." One closes the book with a renewed sense of the arrogance and insensitivity an unbridled rationalism risks.

JOHN HIGHAM

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Public Papers of the Presidents of the United States. Richard Nixon. Containing the Public Messages, Speeches, and Statements of the President. [Volume 1,] 1969; [volume 2,] 1970; [volume 3,] 1971. (Office of the Federal Register, National Archives and Records Service, General Services Administration.) Washington: Government Printing Office. 1971; 1971; 1972. Pp. lii, 1183; liii, 1305; xlix, 1362. \$14.50; \$15.75; \$15.75.

With the publication of the first three volumes of the *Public Papers of Richard Nixon* for the years 1969, 1970, and 1971, respectively, scholars now have at their disposal a well-organized guide for the Nixon presidency that covers everything from Vietnam and Sino-Russian-American diplomacy to the problems of the economy, social welfare, and the White House's relations with Congress and minority groups.

The first volume (1969) opens with President Nixon's inaugural address in which he called for a lowering of voices at home and the need to begin an "era of negotiation" abroad. But the major problems that beset his administration during that first year in office, namely, Vietnam and inflation, made it difficult for him to achieve either goal. On the matter of Vietnam Nixon moved adroitly to neutralize the volatile domestic issue of the war. In order to avoid either a unilateral withdrawal or a settlement on terms that would have alienated

his constituency and damaged the national interest as he defined it, Nixon hatched the plan of "Vietnamization," which he first articulated during his June 1969 Midway Island meeting with President Thieu of South Vietnam (pp. 443-50). On September 16, 1969, the president publicly announced that troop levels in South Vietnam were going to be reduced (p. 718); and on December 15, 1969, he informed the American people that another 50,000 troops would be brought home by April 1970 (p. 1027).

A key to Nixon's Vietnam policy was revealed in his Guam doctrine: on July 25, 1969, he declared that the United States had no intention of playing a passive role in the Pacific basin or in Asia, but in response to the new forces and needs of Asian nationalism it would accept the principle of "Asia for the Asians. And that is what we want, and that is the role we should play. We should assist, but we should not dictate" (p. 548).

On the domestic front Nixon, in 1969, did not ignore the specter of inflation, but he made it clear, time and again, that the imposition of price and wage controls was no solution to the problem. A better tack, he argued, was to reduce federal spending, retain the surtax, and tighten credit controls—all of which, he felt, would restore the economy to a state of noninflationary balance and growth, even though such short-term policies would produce higher unemployment (pp. 808-14). In the area of social services Nixon retained the Office of Economic Opportunity. But more important was his strong endorsement of a comprehensive family assistance program (FAP) to replace the archaic welfare system with something akin to a guaranteed annual income for the working poor and those who were simply unemployable for reasons of health or age or who lacked legitimate job opportunities (pp. 637-45). This program, which Nixon shrewdly tied to a revenue sharing scheme for the states and municipalities, was a landmark proposal, deserving careful consideration from liberals and conservatives alike.

As the 1970 volume reveals, President Nixon's second year in office was far more conflict-ridden than his first, especially in his relations with Congress. Although Congress generally supported his Vietnam policy there

were rifts between the White House and Congress on matters affecting the appropriation of funds for such social welfare programs as OEO, hospital construction and modernization, and education. Nixon vetoed these congressionally sanctioned bills, charging that they made it difficult for him to bring an inflationary economy under control (see pp. 21-26, 513-15, 663-65).

In another area, the president moved to enlarge his political base in the South, a move dictated by his need to head-off or weaken a possible Wallace campaign in the heart of Dixie preparatory to the 1972 campaign. Therefore, Nixon began to speak out in defense of the neighborhood school system and to oppose busing for the purpose of achieving racial balance (p. 315). He also nominated a Southerner for a Supreme Court seat, Judge G. Harrold Carswell of Florida, who, like an earlier Nixon nominee, Judge Clement F. Haynsworth of South Carolina, failed to obtain Senate confirmation. Piqued by this defeat Nixon castigated the Senate by charging that both Carswell and Haynsworth had been victims of "regional discrimination" (p. 346).

Perhaps the people of Cambodia were the more authentic victims of "regional discrimination" following Nixon's order to American troops to liberate territory located inside Cambodia. Escalating his rhetoric to match this precipitate move, Nixon solemnly declared on April 30, 1970, that "if, when the chips are down, the world's most powerful nation, the United States of America, acts like a pitiful helpless giant, the forces of totalitarianism and anarchy will threaten free nations and free institutions throughout the world" (p. 409).

The campus protest against Nixon's Cambodian incursion was enormous, though it led to the tragedy at Kent State University and, later, the killings at Jackson State College. Responding to the profound opposition to his Cambodian policy from college students across America the president created a special commission to examine the causes and origins of student unrest and rebellion and to recommend ways of better protecting academic freedom (pp. 498-99). That commission, headed by former Governor William Scranton of Pennsylvania, submitted its report, which was somewhat critical of his war leadership, to

him on September 26, 1970. Waiting until December 12, 1970, to answer Governor Scranton, Nixon absolved his administration of any responsibility for the campus turmoil of the previous spring. His full statement can be found on pages 1115-21 of the 1970 volume.

Another interesting feature of the 1970 volume, aside from the lengthy State of the World Message to Congress (pp. 116-90), is the complete coverage of President Nixon's active involvement in the 1970 congressional elections. Nixon, once described by Lyndon Johnson as a "chronic campaigner," was off and running from mid-October until election day in early November, seeking support from "the silent majority" for both his law and order program and votes for his party's congressional candidates other than Senator Charles Goodell of New York. But despite his hard work and tireless campaigning Nixon made little progress in changing the composition of Congress. Fortunately for the Democrats, economic worries, inflation, and the fear of unemployment blunted Nixon's efforts to establish a more solid Republican base in Congress, a fact that the president would not forget during 1971.

The 1971 volume is full of continuities, as many of the issues, problems, and programs as well as the rhetoric are carried over from 1970. For instance, in 1971 the president continued his strenuous rhetorical and legislative campaign in support of environmentalism; he once more stressed the need for a volunteer army and again endorsed a basic reform in the Selective Service Act (pp. 75-78). And ever mindful of the racial fears of millions of white Americans he repeated his defense of the neighborhood school system and opposition to busing to achieve racial balance (pp. 848-49). In addition, he made it clear that the federal government would not "impose federally assisted housing on any community" (p. 731). Nixon also continued to support revenue sharing, suggesting that here was the means of "opening the way to a new American revolution—a peaceful revolution in which power was turned back to the people" (p. 54); and the family assistance program was endorsed on all appropriate occasions.

"Vietnamization" was another theme the president orchestrated in 1971 at every opportunity. Pointing out that by the end of 1971,

400,000 American troops would have been withdrawn from South Vietnam, he suggested that the other 150,000 troops still remaining would come home as soon as military and diplomatic circumstances permitted (p. 1101).

What appears to be expressly new in 1971 was Nixon's public commitment to an expansionary federal budget to help bring about a full employment economy (p. 52) and a program for a major reorganization of the federal executive "to focus and concentrate the responsibility for getting problems solved" (pp. 56, 86). Of far greater importance was Nixon's skillfully executed policy of opening the door, once again, to China, culminating with the announcement that he would go to China in early 1972 (pp. 819, 1143). News of a breakthrough in the Strategic Arms Limitations Talks (p. 648) led, in time, to another dramatic revelation, namely, that the president would visit Moscow in May 1972, presumably for the purpose of helping to put the frosting on the cake being baked at Helsinki (p. 1030).

A remarkable shift in the administration's economic policy, having profound domestic and international consequences, was communicated by President Nixon to the American people on August 15, 1971 (pp. 886-90). Among the points made by Nixon were: the need to impose temporary price and wage controls, the establishment of a ten per cent import tax, the abandonment of the seven per cent excise tax on domestic built automobiles, and a proposal to modify and restructure the international monetary system to promote "stability and equal treatment." That speech, which should be read in conjunction with a talk Nixon gave earlier in Kansas City on July 6, 1971 (pp. 802-13), clearly indicated that the president was taking cognizance of the fact that world politics was in a far more fluid state than at any time since 1945 and that he was trying to adjust American power to that change in such a way as to keep his political and economic losses at a minimum.

A final comment is in order: all three volumes are superbly edited, handsomely printed, and are accompanied by well-organized appendixes that provide additional information, trivial or otherwise.

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LATIN AMERICA

CYRIL HAMSHERE. *The British in the Caribbean*. Cambridge, Mass.: Harvard University Press. 1972. Pp. xii, 240. \$12.95.

In its British edition this book forms part of a series dealing with the social history of the British overseas. Its title gives one reason to hope that Mr. Hamshire will continue the work so solidly initiated by Carl and Roberta Bridenbaugh's recent *No Peace Beyond the Line*. Instead, one is given another short history of the British West Indies—hardly more than an outline, as the author himself admits—that is arbitrary in the selection of material, fragmentary in treatment, distorted in structure and emphasis, and markedly inferior to the volume with which it must beg immediate comparison, J. H. Parry and P. M. Sherlock's *Short History of the West Indies*. Happily the latter is still available and at a much lower price.

Mr. Hamshire is most at home when he deals with pre-emancipation society. Nine of his thirteen chapters are devoted to this period, but the reader will find nothing in them that has not been related several times before. Nor will he find a single reference to the many problems in West Indian history around which considerable scholarly heat continues to be generated: the degree of profitability of the plantation economy, the ambiguities of slave insurgence and docility, and the role of religion in West Indian society, to name but a few. Although the book is innocent of footnotes, Mr. Hamshire tells us in a bibliographic note that he has leaned heavily on A. P. Newton, V. T. Harlow, and J. A. Williamson for his account of the sixteenth and seventeenth centuries and on the journals of Lady Nugent, Monk Lewis, and Thomas St. Clair for his picture of West Indian society as it approached emancipation. After that, his sources begin to fail him. He would appear, for instance, to be unfamiliar with such standard works as those of W. L. Burn on apprenticeship, R. W. Beachey on the sugar industry in the nineteenth century, W. Sewell on the condition of postemancipation labor, and Gordon K. Lewis on the modern West Indies. Small wonder that the twentieth century is dealt with in nineteen pages, while the

critical disturbances of the late 1930s receive two hasty sentences.

The numerous illustrations, in contrast to the text, are an unqualified delight.

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DAURIL ALDEN, editor. *Colonial Roots of Modern Brazil: Papers of the Newberry Library Conference*. Berkeley and Los Angeles: University of California Press. 1973. Pp. xiii, 294. \$12.50.

In the opening essay, Professor Charles R. Boxer applauds the many contributions to the historiography of colonial Brazil that have appeared since 1950, when he last surveyed the field. Thus he sets the stage for the articles that follow, all of which were delivered at the Newberry Library conference in November 1969. Fresh from their dissertation labors, these young talents have presented original research pieces, rather than contributions to a predetermined theme. And the criticisms of the discussants at the Chicago meetings have been incorporated into the present version, carefully edited by Professor Dauril Alden of the University of Washington.

If there is a dominant theme in the various essays, it revolves around the ability of the Portuguese government to implement its objectives overseas. In the first essay, "Centralization vs. Donatarial Privilege: Pernambuco, 1620-1630," Professor Francis A. Dutra (University of California, Santa Barbara) explains in detail how the proprietor of that province was able to maintain his prerogatives, despite the centralizing trend elsewhere in Brazil. This was made difficult after 1602 as certain governors-general, out of self-interest, spent much of their time in the Pernambucan capital rather than in Bahia. But the appointment of the *Donatario's* brother to the governorship of Pernambuco in 1620 and subsequently to the post of governor-general of Brazil assured the victory for donatarial privilege—an outcome that also benefited the Crown. Professor David M. Davidson (Cornell University) in the second article, "How the Brazilian West Was Won: Freelance and State on the Mato Grosso Frontier, 1737-1752," argues convincingly that it was primarily state power that secured Portugal's victory over Spain in the western frontier

and not just the vaunted frontiersmen and miners of Brazil. In the geopolitical exposition that follows, the author illustrates a common characteristic of this volume: the expertise of these young historians with the methodology of the social sciences.

The third essay, "The Generation of the 1790's and the Idea of Luso-Brazilian Empire," by Professor Kenneth R. Maxwell (University of Kansas), reveals how certain Portuguese leaders encouraged the Brazilian elite to support plans for an empire in Brazil even before it became a reality in 1808. The slave revolt of 1792 in French America and the Bahian events of 1798 had a sobering effect upon Brazilian whites, some of whom had previously leaned toward republicanism. Moreover, the sugar boom of the 1790s and the favorable relationship with the Portuguese market promoted a mood of accommodation among Brazilians. Capitalizing upon this sentiment, the leadership in Lisbon offered the 1803 project to establish the empire's capital in Brazil. Portuguese nationalists, of course, frustrated the effort; but European events of 1807 favored the reformers, thus facilitating the transfer of the Portuguese throne to Brazil.

Another essay, "The Indian Labor Structure in the Portuguese Amazon, 1700-1800," by Professor Colin M. MacLachlan (California State University, Long Beach) underscores the government's powerful role in developing a labor system for the tropical forest areas of the Amazon. The state had a vigorous lead even during the mission period (1680-1757), further reinforcing its control under the directorates (1757-98). As it turned out, only wealthy entrepreneurs had sufficient labor. Therefore, it was decided at the end of the century to allow the free market to determine the distribution of most of the labor force. The conclusions of this excellent article invite comparison with the Spaniard's methods of labor control.

The two remaining essays, "Free Labor in a Slave Economy: the *Lavadores de Cana* of Colonial Bahia," by Professor B. Schwartz (University of Minnesota), and "A Preliminary Inquiry into Money, Prices, and Wages in Rio de Janeiro, 1763-1823," by Professor Harold B. Johnson, Jr. (University of Virginia), are both exploratory in nature. Armed with

impressive charts and statistics, they leave no doubt as to the value of notarial documents, plantation account books, price lists, and other materials in the reconstruction of social and economic patterns. Schwartz's presentation challenges the standard masters-and-slaves characterization of colonial Bahia, showing that there was much more variety in the economic system. Of the free elements, he chose to describe the role of the cane growers, their aspirations, the terms of their rentals, their treatment of slaves, and so forth. And Professor Johnson makes his point well: Latin Americanists can no longer ignore price history.

This is a model work that Brazilianists can point to with pride.

MARIO RODRIGUEZ

University of Southern California

ROBERT BRENT TOPLIN. *The Abolition of Slavery in Brazil*. (Studies in American Negro Life.) New York: Atheneum. 1972. Pp. xvii, 299. \$10.00.

This study, a welcome scholarly contribution to the expanding body of historical literature on the American slave systems, methodically examines the processes of change through which Brazil moved during the final two decades of slavery. Toplin reviews the familiar external circumstances that indicated the growing universal opposition to the slave trade and slavery: British political and diplomatic pressure; the Civil War in the United States; the discernible movement by Spain to abolish slavery in the Spanish Antilles; and the new intellectual climate and social consciousness that viewed the institution as anachronistic, uneconomical, and highly undesirable. The movement toward gradual abolition within Brazil began with the passage of the Rio Branco law of 1871, but until the late 1880s the movement was largely confined to the upper classes. Toplin shows that the movement was centered in the northeast, traditionally the cradle of Brazilian slavery, and that the Catholic Church played a rather insignificant role in the process of abolition. He emphasizes some of the subtle (and not so subtle) regional distinctions that coalesced to foster or hinder the movement toward emancipation. The abolitionist attack was broad-based, accentuating morality and

justice as well as the deleterious effect of slavery for all Brazilians. To the usual humanitarian appeal they then added the unwarranted power of the slaveholding classes, which "paralyzed the national economy . . . kept Brazil dependent upon single crops and hindered the development of cities and industry [while] the land monopoly of the slave proprietors left the majority of the people landless and poor" (p. 121). By the 1880s, Brazilian slaveholders, on the other hand, could no longer defend slavery as a positive, socially beneficial institution. They stressed nationalism and the rights of property—transparent arguments that became increasingly hollow when the abolitionists began to recruit something representing a popular following and to resort to physical means to free the slaves. Toplin concludes that abolition resulted from the general desire of the government and the slavocrats to avoid nationwide anarchy, not from the ordered, reasoned, deliberative process abolition is so often pictured as being.

The Abolition of Slavery in Brazil, while lacking the rich detail and felicitous prose of Robert Conrad's *The Destruction of Brazilian Slavery, 1850-1888* (1972), substantially demolishes the arguments supporting fundamental distinctions within the American slave systems and lends support to the long-standing doubts of writers such as Stanley Stein, Eugene Genovese, Richard Graham, and Emilia Viotti da Costa. The conditions in Brazil do not seem at all atypical for the general process of disintegration of slave systems in the Western Hemisphere, especially those in the Caribbean and Latin America.

FRANKLIN W. KNIGHT

Johns Hopkins University

HÉLIO SILVA, with the collaboration of MARIA CECÍLIA RIBAS CARNEIRO. 1972: *Guerra no Continente*. (O Ciclo de Vargas, volume 12. Documentos da história contemporânea, volume 11-K.) Rio de Janeiro: Civilização Brasileira. 1972. Pp. 448.

Brazil's proximity to Africa made her a key factor in World War II, and her leaders in 1941 and 1942 found themselves playing roles that had international significance. With much to be negotiated between Brazil and the United

States, strains developed. Army Chief of Staff Góis Monteiro, sometimes excited, annoyed earnest United States military officers who wanted to place American troops in Brazil's northeast. Osvaldo Aranha, the indefatigable foreign minister, argued brilliantly for anti-Axis pan-American unity but denounced what he called "impertinent daily letters from the United States Embassy about economic and military matters." Even Getúlio Vargas, Brazil's calm president, is described as becoming impatient during Finance Minister Souza Costa's mission to Washington to get long-promised arms. "We want to know whether or not it is worthwhile being a friend of the United States," Vargas cabled Souza Costa.

Subjects are dealt with in separate chapters that overlap chronologically. They include: Axis airlines in Brazil, Argentina's pro-Axis position, U.S. relations with the Vichy government, and the possible stationing of Brazilian

troops on the Azores (opposed by Salazar).

A strong point of this helpful book is the inclusion of much material from the Vargas files and from the archives of the Brazilian Foreign Ministry. Furthermore, archives at Stuttgart, Germany, and at the Brazilian Maritime Tribunal are the source of interesting, shocking details given in thirty-seven pages about the sinking of almost thirty Brazilian vessels. The discussion of efforts to settle the Peru-Ecuador boundary dispute is based largely on papers published in the Department of State's *Foreign Relations of the United States*.

Hélio Silva praises Vargas and Aranha and observes: "We Brazilians, even those who . . . participated in the events, like this old reporter, did not have an idea of the greatness of Brazil's position and the stature of some of our statesmen."

JOHN W. F. DULLES
University of Arizona

Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

The following letters have been received in connection with the publication of Ray Allen Billington's article, "Tempest in Clio's Teapot: The American Historical Association Rebellion of 1915," AHR, 78 (1973): 348-69.

TO THE EDITOR:

Ray Allen Billington's article makes several references in its first paragraph to recent discontent in the American Historical Association.

Billington puts this recent discontent in the past tense. He says it "*has been rejected* by the majority of the members" but "*has encouraged* long overdue reforms generally tending toward more democratic procedures" (*italics added*).

I had not been aware that the recent discontent was over. On the contrary. I should like

to ask Professor Billington to explain the factual basis for this assertion.

STAUGHTON LYND
Chicago, Illinois

TO THE EDITOR:

Something might be added to the "Tempest in Clio's Teapot." Moses' version of the Ten Commandments in Deuteronomy 5: 6-21 and Exodus 20: 2-17 seems to differ from that of both Billington and J. Franklin Jameson (p. 369). "Thou shalt not steal" is number seven; "Thou shalt not bear false witness against thy neighbor" is number eight; "Thou shalt not covet thy neighbor's wife" is number nine.

RAYMOND J. JIRAN
Thomas Nelson Community College

PROFESSOR BILLINGTON REPLIES:

Although I am not privy to the inner circles of the American Historical Association, I have the distinct impression that the "discontent" to which Staughton Lynd refers reached a crest in the late 1960s and has steadily declined since that time. Personal opinion can never be exactly measured, but one bit of evidence can be advanced: the handful of the faithful who attended the 1972 business meeting of the association in contrast with the multitudes who swarmed to attack or defend the "establishment" a few years before. Whether I am right or wrong concerning the degree of satisfaction of the members, I share Dr. Lynd's hope that discontent does remain and that it will continue to motivate the reforms through which administrative processes are democratized.

The question raised by Professor Jiran con-

cerning the numbering of the Ten Commandments was posed by others who read earlier drafts of the article, forcing me into an unaccustomed excursion into the mysteries of Biblical scholarship. This venture took me no farther than the *Encyclopedia Britannica*, the *Encyclopedia Americana*, and the *Catholic Encyclopedia*, but this was enough to satisfy me that the order and numbering of the commandments varies amazingly. "There is," says the writer in the *Britannica*, "no agreement concerning the enumeration of the ten commands." He goes on to say that Jewish tradition makes the prologue, "I am the Lord your God," the first command and the prohibition against other gods and images the second; medieval practice and Luther lumped these into one command; Greek Orthodox and Protestant Reformed traditions treat the prologue and prohibition of other gods as the first and the prohibition of images as the second. The *Catholic Encyclopedia* describes at some length the reasons for the division between Catholic and Protestant numbering.

I found what seemed to be the most useful ordering of the commandments in the *Americana* (1964 ed., vol. 8, p. 553), which lists them, "shorn of their elaborate commentaries," with "Thou shalt not steal" as number eight and "Thou shalt not bear false witness against thy neighbor" as number nine.

Others more learned than I may quarrel with my wording, but they will have to take on the authorities I have cited. I bow out of the battle.

RAY ALLEN BILLINGTON
Huntington Library

TO THE EDITOR:

Carl E. Schorske's "Politics and Patricide in Freud's *Interpretation of Dreams*" (*AHR*, 78 [1973]: 328-47) argues that Freud's interpretation of his "Revolutionary Dream" of 1898, which referred to the prince as "the father of his country" from whose "power the other social authorities have developed" in history, foreshadowed Freud's "mature political theory." Its "central principle" was "that all politics is reducible to the primal conflict between father and son." Schorske states, in effect, that psychoanalysis was Freud's "revenge on politics," in that "patricide replaces regicide: psy-

choanalysis overcomes history," and "politics is neutralized by a counterpolitical psychology." Schorske's footnote 40, apparently meant to support this thesis, states: "This theory [i.e., "all politics is reducible to the primal conflict between father and son"] was set forth in Freud, *Totem and Taboo* (1913)." The expression "all politics," however, artificially broadens the scope of Freud's comment on government and law. This was, in any case, a secondary motif in *Totem and Taboo*. Its primary objective was to link the repressed patricidal (oedipal) wish of every boy and the killing of God the Father, "as in the Christian myth," to the totem meal, "perhaps mankind's earliest festival. . . a repetition and a commemoration of [that] memorable and criminal deed [the murder-cannibalism of the tribal sons of their father, possessor of the females] which was the beginning of so many things—of social organization, of moral restrictions and of religion." The mention of religion last should deceive no one. The dramatic message of *Totem and Taboo* was: The crucifixion is a symbol of repressed infantile sexuality.

Psychoanalysis, said Freud, merely confirmed "the habitual pronouncement of the pious: we are all miserable sinners." But the analyst (unlike the priest, who was able only to reinforce the mechanism of repression) could relieve the religion-implemented neurosis by displacing the ambivalent attitude toward the father onto a substitute. But such displacement could not resolve the original ambivalence.

Thus Freud's discovery of psychoanalysis (his self-discovery, too) could not, as Schorske writes, have enabled Freud to "overcome his Rome neurosis," nor could it have reduced his "own *political* [italics added] past and present to an epiphenomenal status in relation to the primal conflict between father and son, giving his fellow liberals an ahistorical theory of man." It was his feeling about religion that Freud reduced to epiphenomenal status.

Freud's Rome neurosis was one of many conflicts that lasted throughout his life. "My emotional life has always insisted that I should have an intimate friend and a hated enemy come together in a single individual," he wrote in *The Interpretation of Dreams*. And he passionately attacked religion—the factor that may well have barred his career in Austrian law

and politics—in many of his medical writings and, of course, in *Totem and Taboo*, *The Future of an Illusion*, *Civilization and Its Discontents*, *Leonardo da Vinci*, and, last but not least—the year before he died—in *Moses and Monotheism*, which extended to the Jews the crime of Father-God murder of the Egyptian prince Moses, who had saddled the Hebrews with his own religion. Thereby he had reduced the entire Judaeo-Christian teaching to “a neurosis of mankind” whose “grandiose powers” were explainable “in the same way as we

should a neurotic obsession in our patients.” Freud’s “ahistorical theory of man” was a definition of monotheism as a societal disorder, which, if abolished by psychoanalysis, would allow the irreligious Freud to enjoy the heritage of Judaism, unmarred by its obsessional forms, and to enjoy Rome (that is, Vienna, capital of the Holy Roman Habsburgs) uncluttered by Catholicism.

STANLEY W. PAGE
City College,
City University of New York

Recent Deaths

EDGAR A. J. JOHNSON, professor emeritus of economic history at the School of Advanced International Studies of the Johns Hopkins University, died from multiple myeloma in Washington, D.C., on August 17, 1972, at the age of seventy-two.

Johnson came from a Swedish immigrant family, which had settled on a farm in Illinois. He received his education at the Orion, Illinois, high school, at the University of Illinois, and at Harvard. His career was varied and peripatetic. His primary teaching posts were successively at the University of Oklahoma, Cornell University, New York University, and the Johns Hopkins University, but in addition he was visiting professor at a number of other institutions.

During World War II, he served as chief of the economic branch of the Allied land forces in Norway and as deputy chief of U.S. supply control in Germany. After the war he was a civil administrator in Korea, economic adviser to the ECA mission to Greece, deputy chief of the United States aid mission to Yugoslavia, and more recently was consultant to the Indian National Council of Applied Economic Research.

In addition to his teaching and public service, Johnson found time to produce a long list of publications, the most important of which were: *American Economic Thought in the Seventeenth Century* (1932); *Predecessors of Adam Smith* (1937); (with Hermann Krooss) *The Origins and Development of the American Economy* (1953); *Market Towns and Spatial Development of India* (1965); *Organizing Space in Developing Countries* (1970); and his extremely interesting autobiography with the unlikely title of *American Imperialism in the*

Image of Peter Gynt—Memoirs of a Professor-Bureaucrat (1971). Two posthumous works, *Foundations of American Freedom* and *Spatial Aspects of Economic Development*, are scheduled for publication.

Johnson was a staunch advocate of a closer intellectual relationship between historians and economists, which accounts, in part, for his role in founding the Economic History Association, of which he was president from 1960 to 1962. As the first editor of the *Journal of Economic History*, established in 1939, he displayed high standards of scholarship and composition. Our personal friendship came from our collaboration on the *Journal*, for I was associate editor and treasurer of the association.

Johnson received many honors for his great variety of activities. He was made a member of the Order of the British Empire (1945); he received the King Haakon VII Cross of Freedom (1945); and he was given an LL.D. by the Johns Hopkins University (1972).

Johnson is survived by his widow Virginia Gravel Johnson and by his son, Edgar A. J. Johnson, Jr.

SHEPARD B. CLOUGH
Columbia University

DOUGLAS R. LACEY, chairman of the department of history at the United States Naval Academy, died July 27, 1973. He was sixty years old. Dr. Lacey attended Illinois College and Rutgers and Columbia Universities. He received his Ph.D. from Columbia in 1959. Joining the faculty at the academy in 1941, he was named chairman of the history department in September 1970. In 1971-72 Professor Lacey was a senior research fellow at the Folger Shakespeare Library, and he spent 1946-47 in Lon-

don as a Rockefeller fellow in the humanities. He had previously taught at Rutgers, the City College of New York, Sarah Lawrence College, the University of Alberta, and the University of Maryland.

HENRY LITHGOW ROBERTS, who died October 17, 1972, at his home in Rochester, Vermont, served for nineteen years (1948–67) in the history department of Columbia University. In addition to holding a chair in the university (from 1954) he was also director of Columbia's Russian Institute from 1956 to 1962 and of the East Central Europe program (which grew into the Institute on East Central Europe) from 1954 to 1967. He was editor of the *Slavic Review* from 1965 to 1967. After leaving Columbia he held a chair in history at Dartmouth from 1967 until his death. Roberts was one of the leading pioneers of East European and Russian studies in America.

Roberts took his B.A. (1938) and Ph.D. (1942) at Yale, specializing in modern German history. War service in the navy and in the OSS widened and deepened his interest in Europe. He spent most of 1945 in Romania as a member of the American component of the Allied Control Commission. In the same year he married Deborah Calkins. While still at Yale he had won a Rhodes scholarship but had been unable to take it up owing to Britain's involvement in war in 1939. He and his wife decided to use this opportunity after the war to spend two years (1946–48) at Balliol, where he took an Oxford D.Phil., developing through systematic research the understanding of Romania that he had acquired during his official service. The doctoral dissertation was remolded into a book, *Rumania: Political Problems of an Agrarian State*, published in 1951. This, his largest published work, remains still the best survey of a subject that is not only interesting in itself but also can be illuminating to all who seek to understand the interplay of political and social forces, of ideas and action in "developing societies." The author's sympathy for the Romanians is clear on every page, as is his awareness of the richly comic and tragic features of their history. He neither took them at their own valuation nor fell for the persuasive arguments of their many enemies, but rather he saw them as they were and how they had grown into

what they were, critically and compassionately.

During his Columbia years Roberts also did much work for the Council on Foreign Relations. For eighteen years he edited the book review section of *Foreign Affairs*. He was also part-author of *Britain and the United States: Problems in Cooperation* (1953) and sole author of *Russia and America: Dangers and Prospects* (1956), both based on study groups sponsored by the Council. In both books Robert's own hand is clearly visible; and of their quality it may perhaps suffice to say that, after all that has happened in the last twenty years in all three countries, they still bear reading. The questions he put may not have been answered, but they are the right questions. During these years he also wrote numerous articles, mainly on aspects of modern Russian history and politics, toward which, under the influence of G. T. Robinson and Philip Mosely, his interest increasingly turned. Some of the best of these were put together in a volume entitled *Eastern Europe: Politics, Revolution and Diplomacy*, published in 1972.

Roberts was outstanding as a historian, a teacher, and an organizer. It is possibly in this last field (broadly understood) that his achievement was greatest—though he would perhaps have been surprised at the thought. He was indefatigable, both in promoting the study of Eastern Europe and Russia in the United States and in helping students and colleagues, both his contemporaries and his juniors. He showed infinite patience, vigilance, and wisdom in the long hours of committee and office work. To a large extent the scholarly achievements of others, whose burdens he assumed, form part of his own achievement. When he left the turmoil of New York for his beautiful Vermont valley he had earned his rest; but the change meant no end to intellectual effort, as his colleagues at Dartmouth and his, sadly, uncompleted work on the Russian Revolution of 1917 bear witness.

Roberts succeeded, as well as any one can do, in seeing the conflicts of twentieth-century history both as an American and as a citizen of the world. The moralizing denunciations of the early 1950s were as alien to him as the moralizing self-flagellations of the late 1960s. This does not mean that he was unaware of the moral dimension in world politics: it was not his style to run away from it behind a smoke screen of

phrases about value-free judgments. He was second to none in scrupulous exploration and verification of historical evidence, but he did not seek in professional devotion an excuse to opt out of the age in which he lived. He never succumbed to the temptation either to lavish unwanted advice on other nations or to bow uncritically before the culture of Europe. From the first attitude he was saved by his moral sensitivity, from the second by his sense of humor.

It has not been easy to write an obituary of my first and best graduate student, who was also

my first and best American friend. But the loss is more than personal; and many others who knew him little or not at all will remain deeply indebted to his efforts and have good cause to be thankful for his life.

HUGH SETON-WATSON
University of London

Others members of the association who have died recently are Joseph M. Carrière of the University of Virginia in Charlottesville, Virginia, and Peter M. Isajiw of Philadelphia, Pennsylvania.

Association Notes

Mrs. Eleanor F. Straub has been appointed assistant executive secretary of the AHA, succeeding Mr. John J. Rumbarger, currently editor of *Prologue: The Journal of the National Archives*. Mrs. Straub, whose appointment began in September of this year, expects to be awarded the Ph.D. from Emory University by the end of this year.

Mr. Edward C. Papenfuse, Jr. resigned his post as associate editor-bibliographer of the *AHR* to become assistant archivist of the State of Maryland. Mr. James Dougherty, formerly a lecturer at University College, University of Maryland, has joined the staff of the *AHR* as assistant editor-bibliographer. In addition to his other responsibilities, Mr. Dougherty will be the director of the new *Writings on American History* project, which has been made possible by funding from the National Endowment for the Humanities.

Mrs. Janet Hayman, who has become editor of the *AHA Newsletter* on a full-time basis, is leaving the staff of the *AHR*, where she has been an assistant editor. Mr. Randall Koladis has been appointed an assistant editor.

Other Books Received

Books listed were received by the *AHR* between May 1 and July 15, 1973. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

GENERAL

ALBRECHT-CARRIÉ, RENÉ. *A Diplomatic History of Europe since the Congress of Vienna*. Rev. ed.; New York: Harper and Row. 1973. Pp. xix, 764. \$6.95.

ARON, R., et al. *Politica di potenza e imperialismo: L'analisi dell'imperialismo alla luce della dottrina della ragion di Stato*. Ed. by SERGIO PISTONE. Società e politica, 1. [Milan:] Angeli Editore. 1973. Pp. 404. L. 3,500.

ASHLEY, MAURICE. *A History of Europe, 1648-1815*. Englewood Cliffs, N.J.: Prentice-Hall. 1973. Pp. ix, 295. Cloth \$8.95, paper \$4.95.

BEN-AMITAY, JACOB. *The History of Political Thought: From Ancient to Present Times*. New York: Philosophical Library. 1973. Pp. xv, 318. \$20.00.

Bibliography of the History of Medicine. No. 6, 1970. DHEW Publication no. (NIH) 72-315. Bethesda, Md.: U.S. Department of Health, Education, and Welfare, Public Health Service, National Institutes of Health, National Library of Medicine. n.d. Pp. vi, 295. \$3.70 postpaid.

BLUMENSON, MARTIN. *Bloody River: The Real Tragedy of the Rapido*. Boston: Houghton Mifflin. 1970. Pp. viii, 150. \$4.95.

BOUWSMA, WILLIAM J. *The Culture of Renaissance Humanism*. AHA Pamphlets 401. Washington: American Historical Association. 1973. Pp. 40. \$1.00.

BREISACH, ERNST. *Renaissance Europe, 1300-1517*. New York: Macmillan. 1973. Pp. xxiii, 456. \$10.95.

BRINGLE, MARY. *Eskimos*. First Book. New York: Franklin Watts. 1973. Pp. 87. \$3.95. Grades 4-7.

CHAGNIOT, JEAN. *Les temps modernes, 1661-1789*. Le fil des temps, 6. Paris: Presses Universitaires de France. 1973. Pp. 311.

CHENG, RONALD, YE-LIN (ed.). *The Sociology of Revolution: Readings on Political Upheaval and Popular Unrest*. Chicago: Henry Regnery. 1973. Pp. xxviii, 334. \$12.50.

CORTADA, JAMES W. *United States-Spanish Relations, Wolfram and World War II*. Barcelona: Manuel Pareja; distrib. by International Scholarly Book Services, Portland, Ore. 1971. Pp. 134. \$6.00.

ELMANDJRA, MAHDI. *The United Nations System: An Analysis*. [Hamden, Conn.]: Archon Books. 1973. Pp. 368. \$7.50.

EUBANK, KEITH (ed.). *The Road to World War II: A Documentary History*. New York: Thomas Y. Crowell. 1973. Pp. xiii, 284. \$2.95.

FINOCCHIARO, MAURICE A. *History of Science as Explanation*. Detroit: Wayne State University Press. 1973. Pp. 286. \$15.95.

FISCHEL, WALTER J. (ed. with an introd. and notes). *Unknown Jews in Unknown Lands: The Travels of Rabbi David d'Beth Hillel (1824-1832)*. New York: Ktav Publishing House. 1973. Pp. 130. \$10.00.

FISHMAN, JOSHUA A. *Language and Nationalism: Two Integrative Essays*. Rowley, Mass.: Newbury House. 1973. Pp. xvi, 184. \$8.95.

GAY, PETER, and WEBB, R. K. *Modern Europe to 1815; Modern Europe since 1815*. 2 vols. New York: Harper and Row. 1973. Pp. xiv, 536, xxiv; xvi, 537-1122, xxxiii. \$6.95 each.

GEORGE, ALEXANDER L., et al. *The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam*. Boston: Little, Brown. 1971. Pp. xviii, 268. \$3.50.

HAVILAND, VIRGINIA. *Children and Literature: Views and Reviews*. Glenview, Ill.: Scott, Foresman. 1973. Pp. 461.

HEIMANN, SUSAN. *Christopher Columbus: A Visual Biography*. New York: Franklin Watts. 1973. Pp. 57. \$4.50. Grades 4-5.

HIRSCHFELD, GERHARD. *The People: Growth and Survival. First Cycle*. Foreword by KENNETH E. BOULDING. Chicago: Aldine Publishing Co., for the Council for the Study of Mankind. 1973. Pp. xxi, 239. \$7.50.

HOLLANDER, A. N. J. DEN (ed.). *Diverging Parallels: A Comparison of American and European Thought and Action*. The European Association for American Studies. Leiden: E. J. Brill. 1971. Pp. 222. 60 gls.

HOLLANDER, PAUL. *Soviet and American Society: A Comparison*. New York: Oxford University Press. 1973. Pp. xx, 476. \$12.50.

- HOLSTI, OLE R., et al. *Unity and Disintegration in International Alliances: Comparative Studies*. Comparative Studies in Behavioral Science. A Wiley-Interscience Publication. New York: John Wiley and Sons. 1973. Pp. xv, 293. \$12.95.
- HUGHES, MICHAEL (ed.). *The Letters of Lewis Mumford and Frederic J. Osborn: A Transatlantic Dialogue, 1938-70*. New York: Praeger. 1972. Pp. x, 493. \$15.00.
- ISSAWI, CHARLES. *Issawi's Laws of Social Motion*. New York: Hawthorn Books. 1973. Pp. 185. \$5.95.
- JOSHUA, WYNFRED. *Nuclear Weapons and the Atlantic Alliance*. Strategy Papers no. 18. New York: National Strategy Information Center. 1973. Pp. vi, 60. \$1.00.
- KATZNELSON, IRA. *Black Men, White Cities: Race, Politics, and Migration in the United States, 1900-30, and Britain, 1948-68*. New York: Oxford University Press, for the Institute of Race Relations, London. 1973. Pp. xii, 219. \$9.95.
- LE ROY LADURIF, EMMANUEL. *Le territoire de l'historien*. [Paris:] Gallimard. 1973. Pp. 542.
- LUKACS, JOHN. *The Passing of the Modern Age*. New York: Harper and Row. 1970. Pp. ix, 222. \$7.95.
- MCGNAMARA, ROBERT S. *One Hundred Countries, Two Billion People: The Dimensions of Development*. London: Pall Mall Press; distrib. by Praeger, New York. 1973. Pp. 140. \$5.95.
- MLECKA, LOUIS F. (comp.). *Famous People: Historical, Biographical Book of Birthdays*. Brooksville, Fla.: the author. 1973. Pp. unnumbered. \$1.95.
- NUGENT, WALTER T. K. *Creative History*. 2d ed.; Philadelphia: J. B. Lippincott. 1973. Pp. 178. \$2.45.
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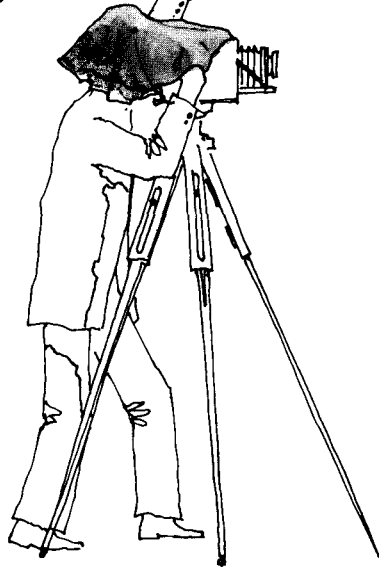
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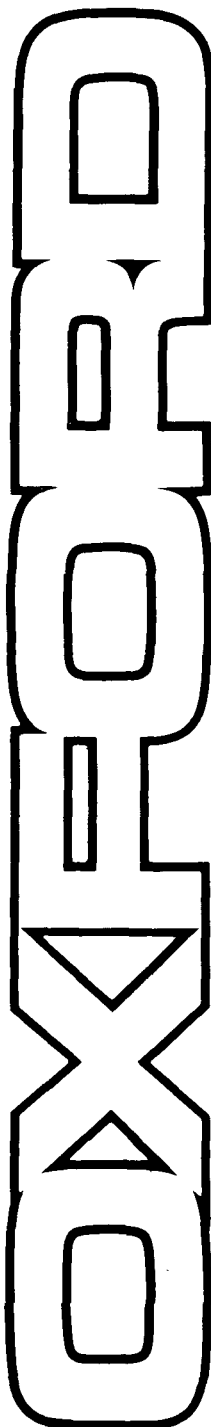
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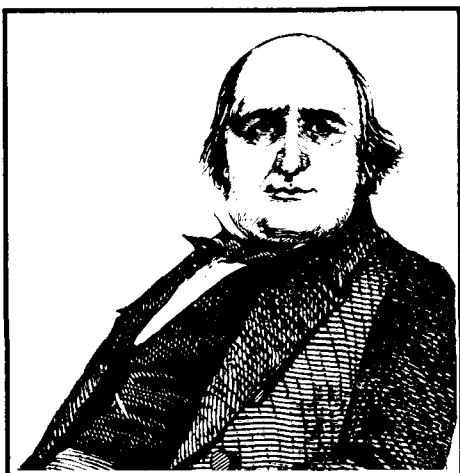
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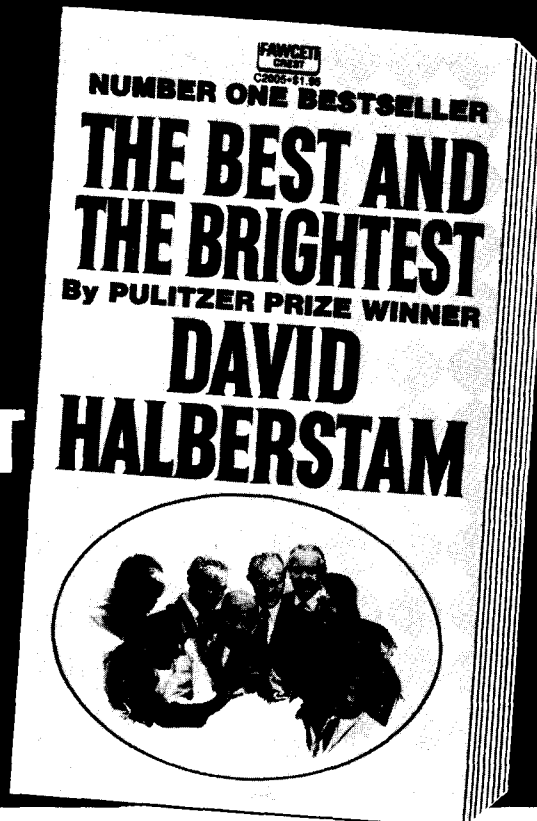
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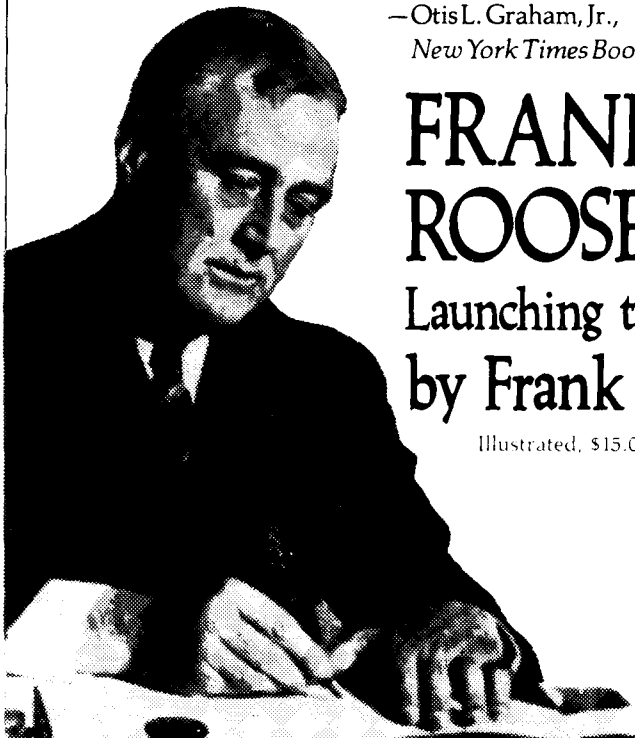
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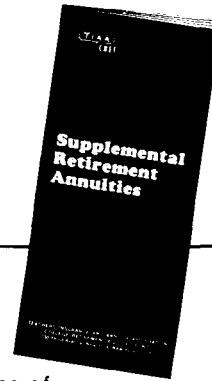
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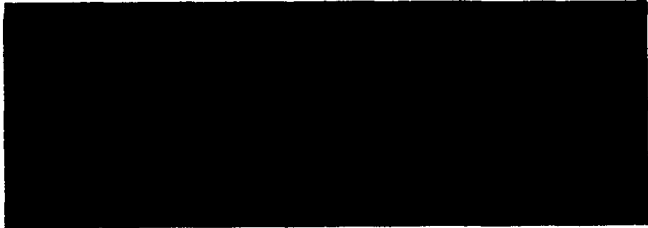
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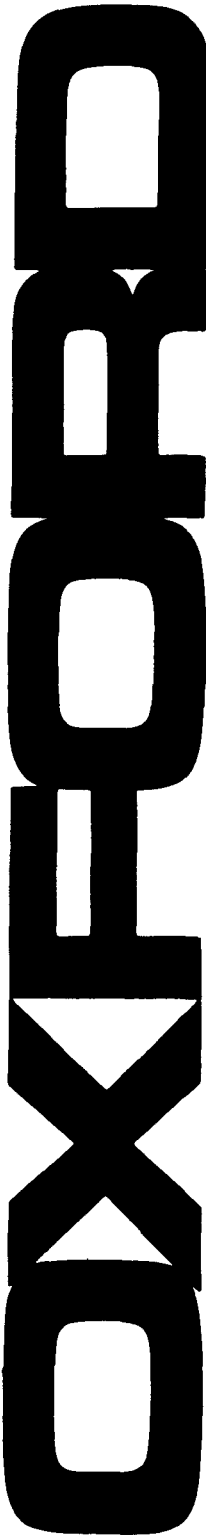
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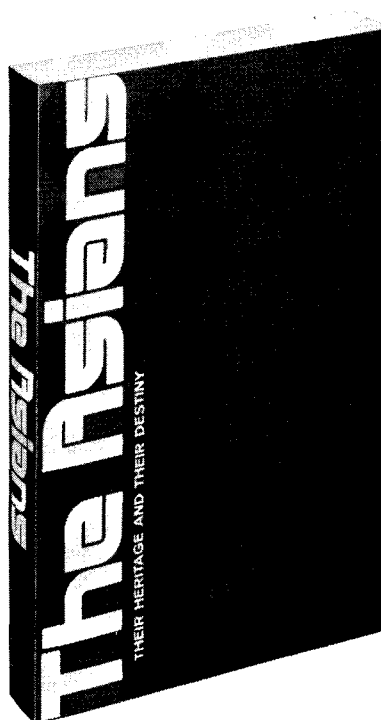
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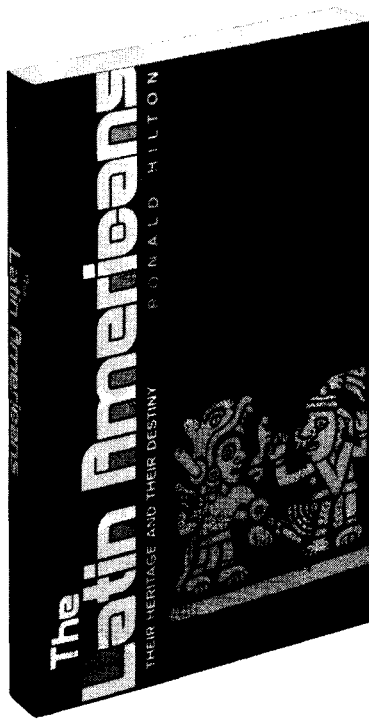
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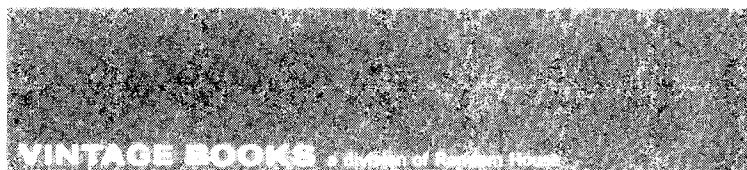
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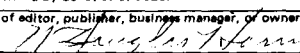
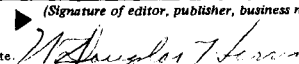
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